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PRIORITIES FOR SPACE RESEARCH 1971-1980

NATIONAL
ACADEMY of
SCIENCES

Priorities for Space Research 1971-1980

Report of a Study on

Space Science and Earth Observations Priorities

Conducted by the

SPACE SCIENCE BOARD

NATIONAL RESEARCH COUNCIL

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Preface

A Study on Priorities in Space Science and Earth Observations was convened by the Space Science Board at Woods Hole, Massachusetts, from July 27 to August 15, 1970. Undertaken at the request of the National Aeronautics and Space Administration, the Study seeks to determine criteria for relative priorities and recommends levels of effort and support to be allocated to NASA programs in planetary exploration, lunar exploration, astronomy, gravitational and solar-terrestrial physics, the environmental science portion of space applications, and the life sciences, during the period 1971–1980, based on likely contributions to basic knowledge and social benefit and on estimated total funding to be available.

Some 90 scientists, including consultants from abroad, were involved during the Study. It was conducted by a 14-member Executive Committee, itself interdisciplinary in composition, in close cooperation with seven Working Groups. Whereas each Working Group was asked to develop programs within three budgetary levels from the point of view of a particular discipline area, the Executive Committee attempted to assimilate the Working Groups' proposals into an overall priority system. To foster a broader outlook, exchange of Working Group membership was encouraged, and a number of sessions were held in which each Working Group presented and discussed its proposals before the entire Study group. In addition to hearing and responding to formal presentations by the Working Groups in plenary sessions, the Executive Committee met with each Working Group for detailed discussions of its program. The discipline chapters in Part II of this report present the views of the respective Working Groups, and Chapters 1 and 2 of Part I present the priorities reached by the Executive Committee.

The conclusions and recommendations of the Study were presented to NASA and other federal officials on August 15. Drafts of the report were sent to all Study participants for review. (A statement by the Study chairman, requested by the Subcommittee on NASA Oversight of the Committee on Science and Astronautics of the U.S. House of Representatives, also drew on draft material from this Study; here the chairman emphasized his responsibility in using the draft material as his personal understanding of the consensus of the study group, noting that the report was still in the process of revision and review.) Finally, within the Academy, the report was reviewed and approved by the Space Science Board and the Committee on Science and Public Policy.

The Space Science Board is grateful to all those who participated in this Study and particularly to its chairman, Herbert Friedman. Appreciative acknowledgment is also owed to the Board's staff in its services to the study committee, and especially to William C. Bartley, who served as Executive Secretary of the Summer Study. The Board also acknowledges with appreciation the support of the National Aeronautics and Space Administration, which made the conduct of the Study possible.

CHARLES H. TOWNES, *Chairman*
Space Science Board

Contents

I	REPORT OF THE EXECUTIVE COMMITTEE	
1	Priorities in Space Science and Earth Observations: Introduction and Summary	3
2	Assessment of Fields under Study and Elaboration of Priorities	23
II	REPORTS OF THE WORK GROUPS	
3	Planetary Exploration	47
4	Lunar Exploration	57
5	Astronomy	71
6	Gravitational Physics	89
7	Solar-Terrestrial Physics	94
8	Earth Environmental Sciences	109
9	Life Sciences	129
Appendix A	Earlier Reports	145

I REPORT OF THE
EXECUTIVE COMMITTEE

1 Priorities in Space Science and Earth Observations: Introduction and Summary

The Study on Priorities in Space Science and Earth Observations was charged with recommending priorities and levels of effort for programs within the purview of the National Aeronautics and Space Administration's Office of Space Science and Applications (OSSA). The Study was fortunate in being able to draw on a number of earlier reports dealing with the several subjects under consideration. They are listed in Appendix A. The programs considered are divided, primarily for OSSA budgetary planning purposes, into seven major areas: planetary exploration, lunar science, astronomy, gravitational physics, solar-terrestrial physics, earth environmental sciences, and life sciences. The period covered is 1971–1980.

Although the task of the Study was to recommend priorities for the 1970's, past experience has demonstrated that such long-range perspectives require continuing short-range review and modification. The pace of discovery and the advance of technical capabilities in some fields has been so rapid that priorities have needed major reassessment before the completion of approved programs. Astronomy is a good example; while only a start has been made in the classical projects of stellar and solar astronomy, strong competition for support has already been developed in such new specializations as x-ray, gamma-ray, and cosmic-ray astronomy, and there is a growing sense of great prospects for infrared astronomy.

Members of the Executive Committee were H. Friedman, *Study Chairman*; L. Carlson, A. Dessler, J. Findlay, J. Firor, T. Gold, R. Goody, K. Greisen, F. Johnson, D. Lindsley, D. Morton, B. O'Brien, L. Schiff, and S. Wolff; and H. Alfvén, J. Blamont, H. Bondi, H. Massey, C. Townes, and H. van de Hulst, *ex officio*.

We therefore *urge* that the assessment of priorities be a continuing process and *recommend* that NASA and the National Academy of Sciences consider how to achieve the most effective coupling between NASA's internal planning and the advice of the Space Science Board. Our present judgments have been strongly influenced by a desire to retain flexibility and responsiveness to new opportunities and to avoid the imposition of rigid long-range constraints on the shape of the total science effort.

The priorities of this Study as presented here in Part I were decided upon by the Executive Committee on the basis of all the evidence and arguments to which it was exposed. Because all highly recommended programs cannot be supported with foreseeable funding, hard choices were forced upon the Executive Committee. The Committee's ordering of priorities reflects its desire to maximize the returns of the nation's investment and to assure appropriate levels of effort in all disciplines. To achieve this goal, some deviation from the ordering of recommendations of individual Working Groups (presented in Part II of this report) was necessary. Thus the views of individual members of the Working Groups may differ from those of the Executive Committee on specific programs. The priorities presented in this Part represent only the views of this Committee.

The Study developed sets of priorities for the OSSA program at three levels of funding—a budget for BASE missions, an INTERMEDIATE budget, and a HIGHER budget.

Among the major issues confronted were those relating to such large-scale efforts as the completion of the Apollo program, Viking, Grand Tour, Large Space Telescope, and earth applications. Ongoing programs were briefly reviewed, and, in some cases, deferrals or stretchouts of missions were recommended in order to accommodate new programs within specified budget constraints. Evaluation of proposals for new starts was emphasized, however, because the Study decided that it was in that area that it could be most useful.

Limitation of the scope of the Study to OSSA programs, while probably necessary in view of the magnitude of that task alone, did nevertheless prevent evaluation of the space program as a whole. Thus, for example, the overall balance between manned and unmanned spaceflight was excluded from the terms of reference. A broad study that would probe the strategy of planning for the entire national space effort would be highly desirable.

A more subtle difficulty somewhat constrained the Study's evaluation of alternative future programs: before a realistic priority can be assigned, the cost of a project must be considered both in relation to its value to

programs within OSSA and in relation to nonspace programs. However, the reliability of cost estimates varies greatly from one project to another. An estimate is reasonably secure only if it can be related to existing projects, e.g., Pioneer F/G, future Apollos, repeated Vikings, or most earth satellites. Costs of new projects can be estimated with varying degrees of reliability, depending upon the approach and the effort expended. For example, considerable effort has been put into estimating the cost of the Planetary Explorer for atmospheric probes and orbiters; less firm figures are available for Explorer floating stations, and even less firm ones for landed packages. The Grand Tour Thermoelectric Outer Planets Spacecraft (TOPS) has received careful attention, but important facets of the mission remain uncertain. There have been cost studies of a next-generation Orbiting Astronomical Observatory (OAO); but virtually no detailed cost studies have been made for an ultimate Large Space Telescope (LST) of 3-m aperture, and estimates must rely on terrestrial experience and extrapolations from OAO. Approximately the same situation exists for automated lunar landers, which relate only distantly to Surveyor and Viking. Some missions, e.g., investigations of small bodies—asteroids and planetary satellites—in the solar system, have been given so little attention that the Study was unable to make an intelligent guess as to their cost.

In many instances, therefore, the Study was obliged to take a hypothetical cost figure, which is stated, and to assign a priority on this basis. In most cases we would wish to change the priority given here to such missions if the cost estimates prove to be significantly far from reality. This fact underlines the need for much more detailed cost studies of high-priority items as part of the continuing review process that we have already urged.

CRITERIA FOR PRIORITIES

The assignment of priorities to diverse fields of science and applications was especially difficult because very few of the ongoing and proposed programs can be characterized as less than good. The Study gave considerable thought to defining criteria of merit. A uniformly applicable set of criteria was agreed upon for pure research, and a different set for applied research. In the latter case, criteria of immediately obtainable social usefulness rated higher than those of research merit, although most applied programs contain important opportunities for acquiring fundamental knowledge. Finally, the Study recognized that its criteria of scientific and technological merit are only one set of a larger body of

criteria, including sociopolitical factors, that govern the ultimate shape of a national space program.

The overall objective that guided the Study was to provide balance of effort to achieve the maximum scientific returns from a wide range of research.

The following criteria of intrinsic and extrinsic merit [partially based on A. Weinberg, "Criteria for Scientific Choice," *Minerva* 1 (2), 159-171 (1963)] guided the Study in setting priorities for space science and applications programs:

In gauging *intrinsic merit*, we asked the following questions:

1. Is the field ripe for exploration? Has new observational technology become ready for exploitation? Is the necessary skilled and dedicated manpower available?

2. Does the proposed research address itself to truly significant scientific questions that, if answered, offer the prospect of rather general implications? Does the research offer substantial promise of opening up new areas and scientific questions for investigation? Do the techniques proposed permit investigation of a new range of parameters or situations with high probability of surprising discoveries?

3. Will the subject area continue to be a focus of fundamental science for many years to come?

With regard to *extrinsic merit*, we asked the following questions:

1. How much will the new program of investigation contribute to broad progress in neighboring scientific disciplines? Is it possible that fundamental laws of physics or fundamental precepts of biology may be challenged by the new discoveries? Will a new picture of nature emerge and profoundly affect the larger body of physical or biological science?

2. Will pursuit of the new program contribute to the general health of science? Will it contribute to the training of engineers, and will their expertise be transferable to other technological areas?

3. Are the engineering challenges and demands on instrumentation such that they would lead to the development of new technologies with broad potential benefits?

4. Will the scientific program contribute to national prestige, defense, international cooperation, education and culture, and future applications? For example, new sensor technology for unexplored spectral ranges almost inevitably is of defense interest and can generate new approaches to environmental applications.

After applying the more fundamental criteria, many *collateral factors* need to be weighed:

1. Are space systems the only or best approach to the problem?
2. Has adequate exploitation of ground-based, aircraft, balloon, or small-rocket resources preceded the step to satellite instrumentation?
3. What are the relative merits of broad participation in "small" projects versus limited participation in "big" systems? (For example, what is the proper balance of small rocket experiments versus satellites; how effective are less-expensive Pioneer- and Explorer-class probes versus complex soft landers?)
4. What are the key factors that establish the most desirable time frame, i.e., short-range versus long-range goals, cost penalties versus scientific advantages of stretching out schedules? How long can a mission be deferred without disbanding highly skilled teams and discouraging the continued commitment of outstanding scientists and engineers?

It is clear that many of the above criteria are most readily applied to fundamental research. When judging earth-resources programs, criteria of social merit and feasibility are dominant. A program that contains clear social merit will most likely be carried forward without additional scientific justification.

SUMMARY OF PRIORITIES FOR NEW STARTS (1971-1980)

In developing priorities, the Executive Committee first identified those new starts that, with existing programs of key importance, would be elements of any viable space science and applications program in the 1970's; these have been *defined* as BASE missions. *It must be emphasized that, although all the BASE missions are considered essential to a healthy space program, they do not represent collectively a program with sufficient guarantees of opening up new areas of investigation.* The estimated cost of these BASE missions plus currently approved missions is close to the present level of funding.

Higher levels of funding are required to permit a better balance between conservative programs and those that force expansion of the frontiers of space science and applications. The Study *defined* INTERMEDIATE and HIGHER budget programs representing, respectively, roughly 25 percent and 50 percent increments to the current OSSA budget. The INTERMEDIATE budget program includes, in addition to the BASE missions, a number of the lower-cost, high-priority new starts. The HIGHER

budget program is a relatively broad-gauged and balanced program of scientific exploration and practical utilization of space for earth observations. It includes the missions in the INTERMEDIATE budget program plus additional new starts of high scientific merit and considerable technological challenge. Several of these projects, as indicated below, will require substantial increases in total funding if they are to be carried out without seriously distorting the program of BASE missions. Other lower-cost missions included here are anticipated to have high priority within a few years; at the present time they are assigned lower priority than those in the INTERMEDIATE budget program because further development is necessary.

The following tabulation summarizes the priorities agreed to by the Executive Committee for the BASE missions, the INTERMEDIATE budget program, and the HIGHER budget program. They are listed by discipline area; their ordering does *not* connote further priority judgments within or among the respective areas.

Again we point out that the priorities differ in some cases from the individual Working Group conclusions.

BASE Missions—New Starts (1971–1980)

Planetary Exploration

Planetary Explorers This is a program of continuing investigations of Venus using Delta-launched, spinning spacecraft. The program proposed for the 1970's includes atmospheric probes and orbiters and eventually, if costs permit, landed packages and floating stations (constant-level balloons). Large atmospheric probes could carry 60 lb of instruments to every level of the atmosphere down to the surface. The science capability would include measurements of atmospheric composition; pressure, temperature, and density; cloud structure and winds; and solar and planetary heat balance. Small probes could examine different geographical locations. Three missions, one with a dual launch, have been proposed for the period to 1978. Dual-entry probes are proposed for 1975, and an orbiter for 1976–1977, followed by another mission either with probes or an orbiter in 1978. It is envisaged that hard landers and floating balloon stations may follow at a later date.

Jupiter Missions In this program spacecraft, possibly extended Pioneer, would be launched to Jupiter to probe its atmosphere, orbit the planet, or fly by, possibly toward one of the outer planets. Principal emphasis in engineering would be to guarantee spacecraft reliability to the distance of Jupiter. A high yield of scientific data on the Jovian

magnetic field and radiation belts can be anticipated; on the physics, chemistry, and dynamics of the atmosphere below the clouds; on gravitational field, heat balance, and such mysterious electromagnetic phenomena as the modulation of the planet's radio emission by the motion of its moon, Io; and on the planet's composition, with bearing on the origin of the solar system. If the HIGHER budget program (see below) is funded, including the Grand Tour, the Jupiter exploration missions would follow in the 1980's. We concluded, however, that a thorough study of Jupiter is, for the near future, the most rewarding objective among the outer planets and will contribute to the experience needed for successful missions to more distant planets at a later time.

Lunar Exploration

Automated Lunar and Planetary Landers Fully automated landers and rovers should provide the basic approach to follow-on Apollo and post-Viking lunar and planetary explorations. Remote-control and sample-return capabilities need to be developed. Design studies and basic development, particularly on promising types of remotely controlled devices, should be started as soon as these are feasible.

*Astronomy**

High-Energy Astronomical Observatories (HEAO) This series of spacecraft opens a vast new range of sensitivity in some of the most fundamental areas of astrophysics. A payload of approximately 22,000 lb, launched by a Titan-Centaur class vehicle, will supply the first major opportunity to use the advanced instrumentation that has been developed for x-ray, gamma-ray, and cosmic-ray exploration. The volume and weight are sufficient to accommodate such instruments as x-ray detector arrays of several square meters in aperture, a gamma-ray spark chamber of 1 m² aperture, and cosmic-ray detection devices in the several-thousand-pound class. Initial flights will utilize spin stabilization; later flights will operate in pointed modes and can accommodate large-aperture, long-focal-length, focusing x-ray telescopes.

Small Astronomical Satellites (SAS) The SAS is conceived as a Scout-launched satellite that will provide short-lead-time responses to rapidly developing new areas of astronomy in all ranges of the spectrum from

* In judging the priorities for solar astronomy, the Executive Committee recognized that a major portion of the support of solar physics rests in the Apollo Telescope Mount (ATM) project, which is part of Skylab I and the costs of which are charged to the Office of Manned Spaceflight. Without this important element of support for solar physics coming from the manned program, our recommended balance of priorities in OSSA support of astronomy would have to be readjusted.

radio to gamma rays. It should serve as an exploratory vehicle for new scientific areas and new instrumentation, preliminary to the design of larger observatory spacecraft, but it is in no sense a substitute for the advanced observatories.

Astronomy Rockets, Balloons, and Aircraft Rockets have been the backbone of ultraviolet, infrared, and x-ray astronomical observations above the absorbing atmosphere; balloons have been very economical vehicles for hard x-ray, gamma-ray, and cosmic-ray studies; aircraft are now becoming key vehicles in infrared investigations. A 100-percent increment in support for rockets and balloons is ranked with highest priorities in astronomy. The increased support for rockets and balloons becomes even more essential if the total astronomy program remains tightly constrained.

Mirror for Large Space Telescope (LST) The LST (3-m telescope approaching diffraction-limited performance to the extent that the best technology will permit) is of first priority for fundamental science, but the mission itself must be deferred to the 1980's for reasons of high cost, advanced technology, and possible advantages of manned attendance. We *urge* an immediate start, however, on a diffraction-limited mirror of at least 1.5-m aperture, because development of this technology requires the longest lead time. This mirror development will be applicable to a 1.5-m space telescope included in the INTERMEDIATE budget program.

Gravitational Physics

The leading contenders in relativity studies are the earth-orbiting gyroscopic experiment under development at Stanford University and the European Space Research Organization's (ESRO's) sun-orbiting satellite, for which we expect the Europeans to seek NASA support. The scientific priority is of the highest order, and the development of technology with a goal of early flight is recommended.

Both experiments require highly advanced technology. For example, the gyroscope experiment requires four very precisely spherical gyroscopes on a satellite in circular polar orbit at an altitude of about 800 km. Extremely low drift rates of the order of 0.001 sec of arc per year are sought (~ 7 orders of magnitude smaller than the best earth-based gyroscopes). It may be possible to achieve this performance in space where the force supporting the gyros is negligible. The ESRO satellite requires drag-free operation (drag acceleration to be less than 10^{-10} cm sec⁻²).

Solar-Terrestrial Physics

IMP KK' and Solar-Terrestrial Probe-A Coordinated observations simultaneously performed at different positions in space are the key to

further progress in the understanding of magnetospheric behavior during magnetic substorms and the processes of interaction between the solar wind and the outer boundary of the magnetosphere. IMP KK' is a mother-daughter system with a spacing matched to the dimensions of the magnetosphere-solar wind interface.

A solar-terrestrial probe would be placed in heliocentric orbit 10 million to 15 million km from the earth. It is designed to observe the solar wind and the interplanetary medium beyond the perturbing influence of the earth.

The timing of these programs should match the efforts of ESRO and of other nations that plan to participate in a coordinated international magnetospheric survey during the period of minimum solar activity, 1975-1977.

Solar-Terrestrial-Physics Rockets and Balloons Continuing support for solar-terrestrial-physics rockets and balloons is needed with an increment of 20 percent to restore the previous level of effort. Maintenance of a high-latitude facility, at least on a part-time basis, is essential for polar-cap and auroral-zone studies.

Data Analysis Solar-terrestrial physics has a valuable storehouse of data and an ongoing program of still active high-data-rate satellites. High priority is given to an approximate doubling of funds for data analysis both to analyze data already in hand and to adequately utilize data from future missions.

Earth Observations

Earth Observatory Satellites (EOS) These satellites have highest priority for meteorology and earth-resources surveys and will contribute to oceanography (sea state and surface temperature), hydrology (precipitation and snow cover), and ecology, including animal migration.

Small Applications Technology Satellite (SATS) These Scout-launched satellites are required to permit rapid (1-2-year lead time) development of new approaches to meteorological and earth-resources work.

Satellite-to-Satellite Tracking Measurements of gravity anomalies are best accomplished by tracking a zero-drag satellite at low altitude from a satellite at high altitude. The data are important for studies of mantle convection.

Expanded Data Analysis from Aircraft Surveys for Earth Resources NASA is presently planning to use its aircraft capability to cover several test sites repetitively. As many as three major test sites may be covered monthly (possibly more frequently) to simulate future space-observation capabilities. We recommend continued support of this program with emphasis on increased data analysis and the development of prototype sensors for operational flight.

Life Sciences

The highest life-sciences priority in space is the search for extraterrestrial life and for a fundamental understanding of life's origins (exobiology). Viking, a soft-landed spacecraft to the surface of Mars, is an exobiology mission in the ongoing approved program of OSSA, and therefore did not enter into priority decisions for new starts. It is of first importance that adequate ground-based research relevant to these exobiology missions be supported.

On the assumption that manned spaceflight will continue, a stronger program on the physiology and psychology of man in space should be planned and executed. Support should be given to the extensive ground-based preparatory research necessary for fundamental studies of biology in space.

INTERMEDIATE Budget Program—Additions to BASE Missions

Astronomy

A 1.5-m Space Telescope The major instrumentation goal of optical astronomy in space is a telescope of the order of 3-m diameter figured as closely to diffraction-limited performance as is technologically feasible. This telescope would greatly extend our ability to observe the most distant galaxies and thus would contribute importantly to cosmology. Its application to most other objectives of optical astronomy will be equally advanced over ground-based observations; it will, moreover, have the added advantage of being able to observe in ultraviolet wavelengths.

A program to orbit a telescope of at least 1.5-m aperture is an INTERMEDIATE budget goal for the mid-1970's. If for fiscal reasons a choice between this mission and the TOPS Grand Tour is necessary, we place a higher priority on the 1.5-m telescope in this decade because of its higher scientific promise.

Orbiting Solar Observatories (OSO), L and M These orbiting solar observatories remain important for solar physics. They should be flown during the next high-activity phase of the solar cycle.

Solar-Terrestrial Physics

Atmospheric Explorers (AE), F and G At altitudes below 300 km, atmospheric drag soon degrades the orbit of a satellite. Yet many of the most important problems in aeronomy concern the 100–300-km region. The Atmospheric Explorer satellites have a propulsion capability that can maintain orbits with perigee as low as 130 km and can

even readjust the altitude of circular orbits. AE-C, -D, and -E are in the approved program; the program should be extended to AE-F and -G.

Cluster A and B A cluster is conceived as four subsatellites in a tetrahedral configuration to provide three-dimensional, simultaneous measurements of magnetospheric interfaces. It is the next step beyond the mother-daughter concept of IMP KK'. A cluster in eccentric orbit with apogees of approximately 15 earth radii would also provide excellent data on the magnetosphere-solar wind interface.

Earth Observations

Earth-Resources Satellite (ERS) Prototype developments of earth-resources satellites are a NASA responsibility to user agencies. Such a prototype will probably be required near the end of the decade.

Synchronous Earth Observations Satellites (SEOS) These geosynchronous satellites are essential for meteorological studies of atmospheric dynamics and rapid cloud motions.

Earth Physics A drag-free satellite should be developed with altimeter capabilities for sea-surface height measurement to an accuracy better than 50 cm and possibly as good as 10 cm.

Life Sciences

To qualify man for longer-duration space missions and to ensure his safety and efficiency during prolonged exposure to the space environment, a broad program of space biomedicine must be carried out. In addition, carefully designed experiments can throw light on basic biological processes by subjecting biological materials to unique variables in the space environment. Where ongoing missions permit the accommodation of life-science experiments, these opportunities should be exploited.

HIGHER Budget Program—Additions to INTERMEDIATE Budget Program

Planetary Exploration

Solar-Electric Mercury Orbiter The next step in exploring Mercury after the 1973 flyby should be to orbit the planet in the late 1970's. Solar-electric propulsion now appears to be the most feasible way to accomplish this mission.

Grand Tour (TOPS) The Grand Tour concept takes advantage of an unusual planetary alignment in the period 1975–1980 that permits spacecraft to swing by Jupiter and use its gravitational field to reach

Saturn and Uranus, where the swingby process can be repeated to reach Pluto and Neptune. A Titan-Centaur-Burner II launch would carry a spacecraft to Neptune directly in 40 years, but the Grand Tour mode could accomplish the mission in as little as 7 years.

While the Study Group recognizes the uniqueness of the natural opportunity and the importance of the planetary observations that could be accomplished, it does not place the TOPS Grand Tour in the BASE or INTERMEDIATE budget level categories because of the impact of its cost on the possibilities for accommodating other highly desirable scientific missions at these funding levels. The collective value of these smaller missions is considered to have higher scientific priority than the Grand Tour.

We are concerned that the greater demand that the Grand Tour places on highly advanced technology and its survivability carries greater risks, and that the entire program, including mission profiles and scientific strategy, are likely to be inflexibly determined before the lessons of the first flight can be applied to successive launches.

Heliosphere/Interstellar Missions A Pioneer-class spacecraft can utilize Jupiter's gravity to turn the spacecraft's trajectory out of the ecliptic and pass over the pole of the sun to explore a new region of space. The Executive Committee *recommends* that this mission be conducted if a similar mission is not flown as a part of the Grand Tour.

Lunar Exploration

Lunar Orbiters An orbital system for the moon, each mission would consist of two vehicles, one in low orbit for gravity and magnetometer measurements and one in higher orbit for data relay to determine accurately the effects of far-side anomalies on the lower orbiter.

Astronomy

Solar Observatories (1 sec of arc) Advances in the study of solar activity and coronal heating will entail the spectrophotometric analysis of small, relatively homogeneous regions and will require a platform similar to ATM (Apollo Telescope Mount on Skylab I) but with 1 sec of arc pointing and the capability to carry long and heavy instruments.

To compensate partially for the delay in advancing the solar observing program to this high-resolution capability, oso pointing capability is being steadily improved. Specifications for oso-J call for ~ 1 sec of arc jitter per 5-min time interval and ~ 3 sec of arc per orbit. Although not so large as the proposed 1 sec of arc solar observatory, these improved oso's will be substantially larger and heavier than the current version (oso-6). Also, doubling the support for astronomy rockets (see BASE

Missions, above) should permit more observations with 1 sec of arc rocket pointing.

Kilometer-Wave Orbiting Telescope (KWOT) Observation at long radio wavelengths with resolution adequate to distinguish individual sources will require a rhombic antenna surrounding an area some 10 km in diameter.

Solar-Terrestrial Physics—Solar-Terrestrial Physics Explorers

Electrodynamic Explorer Similar to Atmospheric Explorer (see above), this powered, low-perigee, adjustable-orbit satellite would be specifically instrumented to study the electrodynamic coupling between the ionosphere and the magnetosphere.

Synchronous Explorer These missions would be built on ATS and ESRO observations of the magnetosphere at synchronous altitude.

Plasmapause Explorer Missions are needed to answer questions regarding the physics of the plasmapause.

Neutral Point Explorer High-latitude missions can be designed to examine the solar wind-magnetosphere interface at and near the neutral points.

Earth Observations

Recoverable Earth-Resources Satellites Satellites from which film may be recovered should be flown simultaneously with standard telemetered Earth-Resources Technology Satellites (ERTS) to obtain coverage of the same areas.

Life Sciences

Improved Biosatellite Development of a new-generation satellite system for basic biological studies in space is needed. However, this development must be preceded by extensive ground-based experimentation preparatory to payload definition.

COMMENTS ON PRIORITIES

The Executive Committee was concerned primarily with matters of appropriate balance in the program. Large complex missions seriously unbalance the program unless a solid base of smaller missions is continued. The risk of such unbalancing is often increased by the dramatic appeal of the large missions to those more concerned with nonscientific justifications. From a management point of view, program choices are influenced by the desire to fit missions to special capabilities of NASA

centers in an effort to preserve the balance of effort in various centers. Nevertheless, we feel strongly that such large missions should not be mounted if they crowd out the smaller missions of high scientific priority. A related consideration is that a variety of exploratory steps generally provides a sounder basis upon which subsequent complex missions can be built. The use of rockets of the caliber of the Aerobee to carry out early, broad-gauged explorations in solar-terrestrial physics and astronomy is the classic example of progress by successive small steps, which ultimately lead to major satellite missions. Because of the high scientific payoff of the rocket programs, we *recommend* continuing and increasing support for such efforts.

Certain fields of science clearly gain more than others from the opportunities of space, although it is not beyond reasonable probability that the balance may shift in a decade. At the moment, for example, astronomy, solar-terrestrial physics, and lunar and planetary exploration gain tremendously greater scope through the use of space vehicles, but fundamental physics in space is represented only by relativity experiments. The life sciences place highest value on exobiology, including the search for evidence of past or present extraterrestrial life; although the evidence from Mariners 6 and 7 has decreased our expectations of detecting life on Mars, a successful life-detection experiment on Viking could swing priorities very strongly in favor of expanded Viking missions. On the other hand, space biology has yet to persuade the community of biologists that it probes the highest-priority questions of life science, and space medicine is required more as a guarantee of the well-being of astronauts in prolonged space missions than as a major means of advancing medical science.

In applications, communications and navigations systems are already proven to be useful. The Study therefore confined its investigation to the earth sciences—meteorology, oceanography, and solid-earth physics—and to earth-resources surveys. Support was recommended for the earth-resources surveys because of the promise of socioeconomic benefits. The priorities question here became more a matter of pacing research and development relative to establishing operations, and to the balancing of the rate of distribution and digestion of information against the rate of acquisition. The meteorology and earth-physics programs have substantial scientific value.

It should be noted that none of the new starts in BASE missions identified by this Study requires the participation of man. The cost of scientific investigations is increased enormously by the requirement to “man-rate” hardware for use in manned flight, quite aside from the great costs of the vehicles and the life-support systems. These comments

are concerned only with the coupling of man to a science program; they do not refer to a manned program *per se*, for there may be entirely valid reasons, unrelated to science, to conduct manned ventures in space.

The absence of recommendations concerning the shuttle and space station should also be noted. These were considered as vehicles for the support of science, but we found the concepts too vaguely defined with respect to costs and engineering difficulties to permit any realistic assessment of the potential values to scientific research and applications. Nor was the Study able to evaluate the economics of the shuttle, because it depends so strongly on the volume of space traffic, which in turn is dependent upon many user activities besides those included in the present frame of reference. It is clear that space science and applications by themselves are insufficient to justify the cost of developing the shuttle.

PREVIOUS STUDIES

During the past several years, a number of studies have been conducted dealing with many of the subjects considered in this priorities study. These reports are listed in Appendix A. The following sections compare major recommendations of earlier studies with the priorities developed by the Executive Committee of the current Study.

Planetary Exploration

Recent studies on planetary exploration include the President's Science Advisory Committee (PSAC) Panel Report on *The Next Decade in Space* and two reports of the Space Science Board: *Venus: Strategy for Exploration* and *The Outer Solar System: A Program for Exploration*.

The emphasis of the Venus report is on modest and relatively low-cost missions described as "Planetary Explorers," which employ Delta-launched, spin-stabilized spacecraft for atmospheric probes, orbiters, hard landers, and floating stations. The recommended mission strategy for the 1970's would utilize the next three opportunities to launch to Venus. The Venus study stresses that great versatility is possible in mission designs ranging from multiple-dispersed entry probes, including surface measurements, to orbiters and constant-level balloons.

The Outer Solar System: A Program for Exploration recommends, in order of priority: (1) a Jupiter deep-entry probe released from a flyby; the flyby would then travel out of the ecliptic and back over the sun at about a 2.5-AU perihelion (1974–1975); (2) a Jupiter orbiter plus a possible probe in 1976; (3) a Grand Tour of Jupiter, Saturn, and Pluto

in 1977; and (4) a Grand Tour of Jupiter, Uranus, and Neptune in 1979. Flyby missions to all the outer planets were preferred as a balanced beginning rather than a concentration on any one or any particular set at the expense of the others. In the event that the full program could not be funded, the report recommended that the emphasis be directed to Jupiter.

The PSAC Panel recommended the gathering of at least preliminary information for each planet and some of their satellites rather than concentration on any single planet, which would hinder the task of gathering the larger variety of information. It therefore endorsed the use of gravitational assist to fly by Jupiter to the outer planets—Saturn, Uranus, Neptune, and Pluto. The PSAC report generally supports the proposals of the NASA Lunar and Planetary Missions Board but suggests that an intensive study of Mars by a lander and rover should be kept an open option until more is known about the nature of the Martian surface.

This priorities study accepts the Planetary Explorer concept as fundamental and considers the possibility of supplements to that program. Within the BASE missions it is not possible to fund the Grand Tour outer-planet missions without jeopardizing the Planetary Explorers and key programs of the other scientific disciplines. Grand Tour missions are also incompatible with funding for our INTERMEDIATE budget program and can be accommodated only at the HIGHER budget level of funding. Therefore, while we recognize the scientific interest in outer-planet exploration, we are led to *recommend* a concentration on Jupiter exploration. On the assumption that approximately \$150 million will be required for Planetary Explorers to Venus, our BASE missions would allow some \$350 million for Jupiter missions. Concentration on Venus and Jupiter, along with the already approved Mars orbiter missions, the Venus–Mercury probe, and the Viking–Mars lander, should provide a very rich yield of planetary science for the decade. At the HIGHER budget level, the Grand Tour missions could be included without distorting the balance of effort in the total program.

Lunar Exploration

The 1969 Space Science Board study, *Lunar Exploration: Strategy for Research 1969–1975*, recommended that NASA's lunar exploration program should focus on optimizing returns on the investment already made in Apollo, and stated that NASA's proposed landing sites for Apollo missions represent a "balanced, minimum list for lunar exploration." Prior to the decision to cancel Apollo mission 20, a number of advisory groups, including the Space Science Board, also reviewed the Apollo

program and recommended full utilization of Apollo's capabilities for scientific exploration of the moon. The Executive Committee of this Study places high priority on the completion of the remaining Apollo flights through Apollo 19.*

Astronomy

The NASA Astronomy Missions Board (AMB) report, *A Long-Range Program in Space Astronomy*, recommended a balanced program in astronomy at two funding levels, a minimum (approximately \$250 million per annum) and an optimum (approximately \$500 million per annum). Fiscal year 1971 OSSA support was about \$80 million for astronomy. Within these AMB plans, there was no explicit attempt to order priorities among optical, solar, x-ray, gamma-ray, cosmic-ray, and radio astronomy. The AMB selected the High-Energy Astronomical Observatory (HEAO) as its first priority new start for 1971. The AMB program, at the minimum funding level, included an increase in support for rockets; a SAS program, growing from one in 1971 to four per year by the end of the decade; HEAO launches in 1974 and 1975; continuation of OSO through L and M in 1977 and 1978. The recommended program for optical astronomy included OAO-D, -E, -F, and -G; and an OXO (Orbiting X-Ray Observatory) based on the OAO spacecraft concept was recommended as a vehicle for x-ray reflecting telescopes. ASTRA (a 1.5-m diffraction-limited optical telescope), with a launch date of 1978, was recommended as the intermediate step to the Large Space Telescope (LST); a 1 sec of arc solar observatory was placed in the same perspective.

The Space Science and Technology Panel of PSAC in its March 1970 report, *The Next Decade in Space*, assigned highest priority to x-ray and gamma-ray astronomy, particularly during early exploration phases. In the face of severe budgetary limitations, it recommended keeping open as many areas of space astronomy as possible by funding smaller projects and avoiding commitments to very expensive programs.

This priorities study *endorses* the high priority of HEAO and the need to increase the rocket program. The full concept of HEAO for the decade includes a pointed version, which replaces the need for the OXO of the AMB. We *recommend* the continuation of OSO through L and M but with some possibility of adjusting the timing so that L and M are accomplished within the solar maximum period that will be centered about 1980. One SAS per year is included, provided it retains the simplicity necessary to

* Subsequent to this study, Apollo missions 15 and 19 have been canceled.

keep its cost at or below \$15 million per spacecraft. Our 1.5-m telescope is essentially the same as ASTRA of the AMB; we *recommend* that work start on the mirror, but the observatory itself can be fitted only into the INTERMEDIATE budget program. The 1 sec of arc solar observatory is placed in the HIGHER budget program. New OAO's are not among our BASE missions, and at HIGHER budget levels we *recommend* the approach to LST rather than continued OAO's.

The AMB minimum program for the decade costs more than we could allocate to astronomy in our BASE missions. The missions included in the BASE and INTERMEDIATE budget programs were all given high priority by the AMB recommendations.

Solar-Terrestrial Physics

The 1968 Space Science Board study, *Physics of the Earth in Space: A Program of Research 1968-1975*, recommended a broad program aimed at understanding the coupling of solar radiation with the environment of near-earth space and the interplanetary medium. Particular emphasis was placed on magnetospheric studies involving "mother-daughter" spacecraft and clusters of spacecraft, spaced to discriminate spatial from temporal variations across various interfaces within the magnetosphere. For aeronomy, development of the Atmospheric Explorer and expanded use of rockets were the principal recommendations. In this priorities study we place in the BASE missions IMP KK', a mother-daughter magnetospheric satellite, and a 20-percent increment in rocket and balloon support. The cluster satellites and continuation of the Atmospheric Explorers through missions AE-C, -D, and -E are placed in the INTERMEDIATE budget program.

Earth Environmental Sciences

An intensive study of *Useful Applications of Earth-Oriented Satellites* was conducted in 1967-1968 by the Division of Engineering of the National Research Council. It generally expressed great confidence in the ultimate benefits of earth-resources surveys. It recommended an immediate satellite program designed to supply pictorial information that would serve as a basis for evaluating future operational problems and systems. Intensive research was urged in remote sensing and in data handling, supported by continuing tests, leading to a fully operational system over a time span of 10 to 12 years. The following quotations are cited from that report: "The advent of multisensor systems tends to aggravate an already troublesome data rate and data-handling situation.

A critical need is the development of new techniques for storage of massive amounts of qualitative data on-board . . . It is generally possible now to produce more information than can be assimilated into the socioeconomic system. First generation systems, which are recommended, are intended to produce photographic printouts. Second-generation systems would rely more heavily on analytical techniques . . . although a considerable research effort will be necessary to develop such analysis techniques."

In this priorities study, the Executive Committee accepted the forecasts of high socioeconomic benefits that the NRC study made for earth-resources surveys, but we question the technical feasibility of discriminating by spectral and spatial resolution and temporal changes such phenomena as crop varieties, crop conditions, forest species, and animal populations. We were surprised at the paucity of information concerning discrimination that seems to have come out of the aircraft program. Accordingly, while we *recommend* a moderate rate of earth-resources satellite launches, we *urge* increased attention to data analysis from aircraft programs and research into data management.

We assign high priority to earth-physics studies in our BASE missions. The significance of the science concerned with continental drift, mantle convection, and oceanography (temperature distribution and ocean currents) and the technical feasibility of achieving the required measurements from satellites combine to make earth physics a highly rewarding area of space research. The present study supports most of the conclusions of the Williamstown Study Report on application of space and astronomic techniques, *The Terrestrial Environment: Solid-Earth and Ocean Physics*, sponsored by NASA.

Life Sciences

A Space Science Board (SSB) study to review NASA life-sciences programs, *Life Sciences in Space*, was conducted immediately before this priorities study, and its preliminary results were available to us. Its first *scientific* priority was assigned to exobiology in view of that field's fundamental scientific importance rather than specific programs designed to implement its study. It stressed the need for a broader program of space biomedicine to "qualify man for space." Finally, it recommended continued extensive support for ground-based fundamental studies in space biology and urged that only definitive experiments be given preference for flight.

The Life Sciences Study drew on a number of earlier studies dealing with facets of the space life sciences, among them PSAC's *The Biomedical*

Foundations of Manned Space Flight (1969) and the SSB's 1969 Santa Cruz Study, *Space Biology*. It generally concurred in the findings and recommendations of these studies, in the case of PSAC, to "qualify man for space," and, for *Space Biology*, to stress also ground-based work and ecological studies from space.

This priorities study also gives highest position in the life sciences to exobiology, which is accommodated by the already approved Viking missions. The Executive Committee *recommends*, however, no initiation of follow-on missions until the results of the 1975 Viking-Mars missions are evaluated. We also agree with the Life Sciences Study in the recommendation of support for ecological studies (see Earth Applications) and for ground-based research in fundamental biology and in the physiology and psychology of man in space. We do not include a biosatellite in the BASE or INTERMEDIATE budget program, but we support the planning of an improved biosatellite in the HIGHER budget program. Space biomedical experiments are recommended for consideration where they can be accommodated in ongoing missions.

2 Assessment of Fields under Study and Elaboration of Priorities

PLANETARY EXPLORATION

The development of the deep-space probe in the 1960's gave rise to a major change in the nature of planetary studies: from a branch of astronomy that had no prospect of obtaining detailed data, planetary studies have developed into a close analog to terrestrial studies. Mariner spacecraft have explored the atmosphere of Mars at all levels and the atmosphere of Venus below the cloud tops to an altitude of about 35 km. Similarities and dissimilarities with the earth's atmosphere have raised fundamental new questions about all planetary atmospheres. Against a background of increasing knowledge of the moon, high-resolution photographs of the Martian surface show that these two bodies must have evolved in very different ways.

The currently approved programs will step up the pace of planetary exploration in the early 1970's. Orbiters in 1971 should answer many questions about the Martian meteorology, possible biological habitats, and planetology. In 1973 and 1974, Pioneer-class spacecraft will be launched to fly by the asteroid belt and Jupiter. In 1973, the remaining inner planet, Mercury, will be photographed and its environment studied from a Mariner flyby. In 1975, a soft-lander, Viking, is to explore the biology and surface properties of Mars. No missions beyond these are approved, and new starts will be necessary to continued progress in exploration of the planets.

The reason for planetary exploration lies first in man's urge to know and understand the world in which he lives. How did the solar system form, and how can we account for the great variety of such celestial bodies as planets, satellites, and asteroids? The planets that are sisters

to the earth must surely have a special place in this search for knowledge. How did our home in the universe come into existence? Can we understand the process, and can we conclude whether it is likely that a similar process took place in the neighborhood of large numbers of other stars? How did life originate? Did it evolve spontaneously and uniquely on the earth, or did it also start, and perhaps die out again, on other planets in the solar system? We believe that the planets may be the result of an accumulation process. Were there earlier phases with different bodies, and did those have life? Is terrestrial life unique, or is it just one example of a universal process? These great questions or similar ones have occupied men's minds since the beginning of history. We have now the opportunity to shed much new light on these problems and perhaps to find a definitive answer to some of them.

The Priorities in Planetary Exploration

First looks at remote bodies should be accompanied by efforts to obtain detailed understanding of more accessible objects. We must increase our superficial knowledge of the surface, lower atmosphere, ionosphere, and plasma environments of Venus and Mars. Investigation of the outer planets must begin with the nearest and largest of them, Jupiter, followed by first looks at one of the outermost planets. Modern technology allows us to think in terms of entry probes, orbiters, and landers and of the many questions that might be answered with these tools. Finally, the 1970's may see the first efforts to investigate comets, asteroids, and other important small bodies in the solar system.

We offer no change in the plans, now well advanced, for the Viking 1975 dual orbiter-lander mission to Mars. The principal justification of this mission is exobiology. It has received a great deal of attention from the scientific community during the last three years and is now a NASA-approved program. Therefore, we did not re-examine in detail either the general plan or the science payload of Viking.

We did consider the merits of future Viking-class planetary landers in relation to the broader approach to solar-system studies, and concluded that initiation of future Mars soft-lander missions should be deferred until returns from Viking '75 are available. However, the study of missions featuring sample return should be initiated immediately in coordination with the lunar automated lander program.

1. Planetary Explorer

This program for Venus emphasizes a strategy of minimizing complexities in spacecraft, scientific experiments, and launch operations. As

discussed in an earlier Space Science Board (SSB) study, *Planetary Exploration 1968–1975*, the Planetary Explorer is envisaged as the prime vehicle for a diagnostic exploration of Venus by means of orbiters, atmospheric probes, floating balloon stations, and hard landers and is designed to be as low in cost as possible. A cost of \$130 million has been estimated for an initial three-mission, four-launch series to Venus by a recent SSB study, *Venus: Strategy for Exploration*. The Executive Committee feels that these missions promise great scientific returns for a relatively modest investment and has, therefore, placed them in the BASE program. This priority should be reassessed if the projected costs increase substantially. Floating balloon stations and hard landers, additional to the initial program, may also be of major importance for planetological and meteorological studies, but their priority relative to other programs depends upon their cost, which should be estimated as soon as possible.

2. *Jupiter Orbiter*

In this program, spacecraft (perhaps Pioneer-type) would be launched to explore Jupiter in detail. To minimize costs, the mission design should aim for reliability and freedom from failure only to the range of Jupiter, but the spacecraft could be permitted to fly by Jupiter and toward Saturn if this is feasible. Since no unique natural opportunity is involved in Jupiter missions, it is possible to pace a program to accommodate available funding.

The cost of an Extended Pioneer orbiter is not well defined. The cost of the current series of Pioneer F and G deep-space probes, which will fly by Jupiter in 1974 and 1975, is somewhere near \$50 million per launch. Addition of an orbiter capability and a Jupiter atmospheric probe might double or triple the cost. However, we believe that individual Extended Pioneer missions to Jupiter would still cost significantly less than the individual missions of the proposed Thermoelectric Outer Planets Spacecraft (TOPS) Grand Tour discussed below. We *urge* NASA to study minimum-cost missions that can carry modest but diagnostic instrumentation to Jupiter and possibly to Saturn within a funding level of about \$350 million for the decade.

3. *The Grand Tour (TOPS)*

This project would explore Jupiter, Saturn, Uranus, Neptune, and Pluto. There will be special opportunities in the late 1970's for spacecraft sent near Jupiter to use that planet's gravitational field to speed them to the outer planets. Possible missions include (a) an initial spacecraft to approach Jupiter and then to swing back toward the sun in a path out of the ecliptic, followed by (b) two launches to fly by Jupiter, Saturn,

and Pluto, and (c) a final two-spacecraft visit to Jupiter, Uranus, and Neptune. Four of these spacecraft—(b) and (c)—would ultimately leave the solar system.

The Grand Tour would give the first exploratory look at the outer planets and many of their satellites; it would make measurements over large distances in interplanetary space and to beyond the solar system. Photographs of the planets and satellites taken on these missions could be hundreds of times better than those taken even from an observatory in earth orbit. Other experiments could give data on the environments of the outer planets comparable in value to those on Mars obtained by the 1969 Mariner Mars mission. More optimistic estimates of performance suggest that, in addition to extended observations of the outer planets at high resolution, the Grand Tour series can achieve long-term, high-resolution observations of 29 planetary satellites in the outer solar system and provide information on additional asteroid belts, satellites, and comets, on the location of the heliopause, and on the nature of the interstellar medium.

The Grand Tour must be launched between 1975 and 1979. A cost estimate of \$700 million for five missions was provided by NASA for this Study, and the Executive Committee feels that, at this cost level, these missions are of high scientific merit in a HIGHER budget program.

4. Small-Bodies Missions

It has been argued that missions to comets and asteroids could lead to new ideas about the formation of the solar system. These arguments are persuasive, and we believe that it may be desirable to mount missions to small bodies in the next decade. However, the project has not been subjected to a sufficiently searching and critical examination by a wide segment of the community, and we believe that this must be done before detailed plans can be advanced and priorities established.

5. Solar-Electric Mercury Orbiter

The logical next stage of Mercury exploration after the 1973 flyby would be carried out by an orbiter with high-resolution imaging. This mission might not be costly once solar-electric propulsion is developed. We have included the mission in the HIGHER budget program rather than the INTERMEDIATE because the propulsion system has not yet been fully proved.

6. Follow-on Viking Landers

The first Viking landers in 1975 could be followed by a series of follow-on missions at approximately 2-year intervals, each costing about

half as much as one of the 1975 landers. It is our view, however, that the high cost of these missions and major uncertainties about the biological results dictate a policy of caution: We should wait to examine results from the first mission before determining the shape of future missions in this series. It would, however, be appropriate to carry on a modest effort toward the development of certain long-lead-time items before Viking '75 results are in.

7. *Level of Effort*

We believe a well-balanced program would continue to explore the nearer planets in depth, taking advantage of ground measurements wherever possible (*Planetary Astronomy: An Appraisal of Ground-Based Opportunities*) and, at the same time, would send exploratory missions to more distant planets. It is possible to develop such a program at different levels, and in this connection some general principles can be stated.

Because the Planetary Explorer is a BASE mission, these flights should be protected from encroachment by the large expenditures needed for large programs. If such large programs cannot be funded at all, it is still possible to devise an interesting and useful plan based on smaller and simpler spacecraft. However, it is also clear that, given only these spacecraft, some tasks, such as the search for evidence of life on Mars, become very difficult.

The possibility that even the 1975 Viking missions would be lost for lack of money must also be considered. In this event it would be very difficult to make a satisfactory study of the Martian surface, although it is possible to consider adapting the Planetary Explorer approach to orbit the planet, probe its atmosphere, and conceivably accomplish a hard landing.

LUNAR EXPLORATION

The goal of the Apollo program was to land a man on the moon and return him safely to earth. This technological goal has been achieved, and it is now time to exploit this technology for scientific goals. We believe that the Apollo program offers a unique opportunity to answer fundamental questions about the origin and evolution of the earth and the solar system. Evidence from Apollo indicates that the age of certain lunar rocks is greater than 4.0 billion years and may approach the 4.6 billion years now generally accepted to be the age of the earth and the solar system. Yet the age of the oldest minerals so far known on

earth is only 3.6 billion years. There is probably no geologic record to be found on the earth for the first billion years of the solar system due to obliteration by terrestrial metamorphism, magmatism, and erosion. Thus a primary goal of the continuing Apollo program should be to explore the moon in order to elaborate the early history of the solar system.

Evidence from Apollos 11 and 12 indicates that the moon has not undergone, at least to the same extent, the severe evolution that has reset the atomic clocks of earth rocks, and that the record of lunar events during the first billion years is preserved. By examining this record, an improved early history of the earth and the solar system may be obtained. This knowledge, used to develop an improved model of the early differentiation of the earth, will lead to the prospect of a better understanding of the metallogenic and geochemical province of the earth.

It may be that there are in the solar system other bodies that have features as old as or older than those of certain lunar regimes, but the fact remains that the moon is the only planetary body accessible enough to the earth to permit exploration in sufficient detail to reconstruct the early history of the solar system. This history can be reconstructed only if visits are made to a sufficient diversity of lunar sites to allow scientists to place the returned samples in their proper geological, physical, and chemical contexts.

Priorities for Lunar Exploration

1. *Apollo*

We *recommend* that the six* remaining Apollo vehicles be utilized for lunar exploration: from a scientific viewpoint, this use of the Apollo system warrants higher priority than diverting the system to other missions such as Skylab or post-Skylab A earth-orbital flights. Moreover, we consider it essential that the manned rover be utilized on the remaining lunar flights. The Study recognizes that the safety of astronauts must always be a central element in decisions regarding manned missions, and the high priority placed by this Study on the completion of the Apollo series must in no way conflict with basic considerations of human safety. We are not in a position to evaluate the complex problem of

* This Study was conducted prior to the NASA decision to eliminate two of the six scheduled launches—Apollo 15 and 19. Nevertheless, we consider it essential to state for the record how scientists felt at that point of decision. Reduction in the number of flights makes it even more important that the remaining flights carry rovers.

astronaut safety. On the basis of elements that we can evaluate, however, we believe that failure to use the investment already made in Apollo systems for lunar exploration will amount to a waste of national resources and will greatly diminish the scientific significance of the Apollo program.

The goals of this continued program should be: (1) to discover the record of the early history of the earth-moon system and to use it as a focus for study of the early evolution of the inner planets and the solar system, (2) to compare surface processes and physical evolution on the moon with those that have affected the earth, and (3) to use the knowledge of the segregation of the moon into different chemical and physical provinces as a model for similar processes on the earth and other terrestrial planets.

2. *Automated Landers and Rovers with Remotely Controlled Experiments*

In an orderly exploration of the solar system, the moon should also be a testing ground for experiments and equipment designed to investigate other planets. In this connection, remotely controlled systems should be given strong consideration, for such equipment with TV imaging would open many currently inaccessible sites to exploration. Thus we *recommend* that testing and development of remotely controlled systems be initiated.

These investigations could include *in situ* soil composition measurements and emplacement of geophysical stations. The landed payload should include a remotely controlled rover to extend areal coverage and possibly the capability for sample return. Although commitment to this program should await the runout of the Apollo program, modest development work should be undertaken at an early date. If the Apollo program is curtailed, this development schedule should be accelerated.

3. *Orbiters*

The second new start should be a simple pair of orbiters designed to map the gravitational field of the far side of the moon. Two vehicles are required, one at low altitude to sense relatively small-scale gravitational anomalies such as mascons and one at high altitude to serve as a data relay for tracking the low-altitude satellite when it is hidden by the moon.

4. *Level of Effort*

Important efforts in instrument development and scientific interpretation of lunar measurements are currently being supported at a level of \$8 million per year. We *recommend* that this funding be continued and modestly increased to offset termination of Apollo-program support of scientific analysis of lunar data as the flight program is completed.

ASTRONOMY

Studies from space and the ground have produced a revolutionary advance in astronomy. The discovery of quasars, pulsars, and the microwave background radiation of the universe has greatly modified our understanding of the structure and evolution of the universe as a whole. The discovery of complex molecules in interstellar space has implications for the origin of life and the formation of stars and planetary systems. Similarly, the observations of the chemical composition and isotopic composition of the cosmic radiation have become an important tool to test models for the synthesis of elements in a variety of stellar objects, particularly in supernovae.

Studies from space make a unique contribution to astronomy and astrophysics because they permit measurements in regions of the spectrum that are inaccessible from the ground. Space measurements in the ultraviolet have yielded a new picture of the evolution of young stars; measurements at still shorter wavelengths have revealed unexpected x-ray, and possibly gamma-ray, sources; measurements in the infrared are showing this region to be an extremely important one for understanding the energy distribution of galaxies. Future observations can be expected to make equally significant contributions. Measuring the cosmic-background radiation in the far infrared is important in the understanding of the early history of the universe. The detection of ultraviolet absorption from interstellar matter would provide information on the structure of the galaxy and its halo. The existence of x radiation from a hot intergalactic plasma would be important to our understanding of the evolution of the universe.

The total energy generated by quasars and radio galaxies has led to questions about the completeness of our understanding of the fundamental law of physics. The study of the cosmic-ray particles bombarding earth from space may well give information on the presence of anti-matter in the universe and certainly will provide us with the opportunity to study the detailed interactions of matter at energies higher than we can hope to duplicate in the laboratory. The study of astrophysics thus has microcosmic as well as macrocosmic consequences and should contribute significantly to our understanding of those fundamental laws of nature that govern the behavior of matter, not only on the vastest imaginable scale but also on the smallest of scales, and the practical importance of this understanding may well be as great in years to come as is that of nuclear physics today.

Astronomical observations from the ground and in space have become more and more interdependent; thus, neither of these facets of the

science can reasonably be pursued without regard to the other. It has been demonstrated in case after case that observations in every wavelength, from long-wave radio to gamma and cosmic rays, are deeply interrelated, and that all must be pursued in order to understand any one of them well. Space platforms are essential to many investigations because the earth's atmosphere distorts the images of telescopes even in visible light and blocks observations in most other spectral regions. Nevertheless ground-based observations importantly complement observations from space, and NASA can appropriately—and should—support selected ground-based programs to derive maximum return from the investment in space missions.

Priorities in Astronomy

The recommendations for the 1970's in astronomy closely reflect the above perspectives, modified only by the need for patience and compromise in financially constrained times. Outstanding science, based on extensive theory, is waiting to be done in many directions and in all parts of the spectrum. We believe that the greatest efficiency and reward lie in supporting developments throughout the spectrum. As a general guide, we *recommend* that highest overall priority be given to maintaining the breadth of spectral coverage, rather than concentrating all available resources on a single dramatic instrument or narrow spectral band.

1. High-Energy Astronomical Observatories (HEAO)

Highest in specific mission priority is a high-energy orbiting astronomical observatory (HEAO) to study x rays, gamma rays, and cosmic rays. These areas of research are now ripe for the major advances to be expected from the massive, large-volume payloads that HEAO could carry. Two missions—one scanning and one pointed—are essential. The payloads of the second spacecraft for each mode could be supplemented by detectors for spectral ranges not covered by the first pair, including the ultraviolet (uv) and infrared (ir). The potential scientific return from the HEAO program would justify spending up to about \$350 million in the next decade in the framework of BASE missions at the present NASA budget level; this limit should govern the number of missions.

2. Rockets, Balloons, Small Astronomy Satellites, and Aircraft

As a base for all astronomy programs, highest priority must also go to increased support for rockets and balloons. We *recommend* a doubling

of the present funding to provide inexpensive, short-lead-time investigations of all regions of the electromagnetic spectrum as well as observations with high angular resolution of the sun (rockets) and planets, nebulae, and galaxies (balloons). Similar in priority is a continuing series of small astronomy satellites (SAS), again to cover all regions of the spectrum. These instruments, together with aircraft infrared measurements would provide new basic data on planetary, galactic, and extragalactic sources at relatively low cost with enough flexibility to incorporate new developments within 2 years. For a minimum program, approximately one SAS launch per year is required, with an average annual budget of \$10 million to \$15 million.

However, such small satellites should not be considered as substitutes for HEAO and the large space telescope (LST), described below, which are needed to study faint objects. Starts on these major projects should have priority over new starts on small satellites for similar energy ranges. Only if the budget prohibits starting either HEAO or LST in this decade would the appropriate SAS receive first priority. It should be noted that small satellites, if funds permit, could also provide important complementary observations for large facilities.

3. *Optical Space Telescopes*

Following the top-priority items is an optical telescope for high angular resolution and uv spectroscopy to study planets, stars, interstellar gas, and galaxies. A diffraction-limited mirror of even 1.5-m diameter could make critical contributions to questions of cosmology, the nature of galactic nuclei, and the composition of the galactic halo. In the long term, a large optical telescope and HEAO have roughly comparable scientific priority, but because optical astronomy is now benefiting from the series of Orbiting Astronomical Observatories (OAO), only the HEAO missions are included in the BASE program. As money becomes available following the new start on HEAO, the optical telescope should proceed in a best effort consonant with a total cost of \$400 million, with a view to launch in this decade. This mission would carry a mirror with a diameter of 1.5 m or larger and figured to diffraction-limited performance with imaging to 0.1 sec of arc or better, high- and low-resolution uv spectrographs, and suitable ir instrumentation. Such an instrument would be a valuable scientific tool and a test for systems to be used in the LST contemplated for the 1980's. As part of the BASE missions, supporting research and technology (SR&T) funds should be used for immediate starts on a development program for figuring and testing the 1.5-m or larger diffraction-limited mirror made from one of the new low-expansion materials such as CerVit or ULE quartz.

4. *Orbiting Solar Observatories (OSO) and Solar Telescope*

Also of high priority in an INTERMEDIATE budget program is the continuation of the series of Orbiting Solar Observatories through OSO-L and -M with improved performance, provided they do not exceed about \$35 million each. These instruments would observe the sunspot maximum conditions toward the end of the decade. A design study also should be started for an automated solar telescope, with a resolution of 1 sec of arc or better, to study the detailed structure of the solar atmosphere at various wavelengths. If development of such an instrument is feasible within a budget of \$150 million, it should be included in the HIGHER budget program and could eliminate the need for OSO-L and -M.

5. *Kilometer-Wave Orbiting Telescope*

Following the solar telescopes in priority in the HIGHER budget program is a rhomboidal kilometer-wave orbiting telescope (KWOT) to observe radio emissions down to 100 kHz from galactic and extragalactic sources. This antenna would be launched at the end of the decade. A final decision on the importance of this project should await results from the Radio Astronomy Explorer (RAE) to be placed in lunar orbit, where occultations will show whether any discrete sources extend to these low frequencies.

6. *Infrared Telescope*

We also *recommend* study of an ir orbiting telescope of the 70-cm class, which might be flown toward the end of the decade. As in the case of the planetary Small Bodies Missions discussed earlier, this mission must be more thoroughly studied before it can be assigned a priority.

7. *Ground-Based Observations*

All aspects of astronomy—ground-based and space-based, observational and theoretical—are interrelated. No one aspect should be allowed to suffer relative to any other to the extent that basic scientific progress in the entire discipline is weakened. Ground-based observations that directly complement and support specific space observations could be funded as essential elements of the space missions up to about 5 percent of the cost of those missions. This would amount to about \$60 million for the decade.

GRAVITATIONAL PHYSICS

Fundamental physical theories provide the basis for all natural sciences and technology. One of the phenomena known since ancient times,

which has been a source of fascination and of utility ever since, is gravitation. For more than 200 years, until early in this century, classical Newtonian theory held sway. This theory is now known to be not entirely valid, and determination of the correct theory of gravitation is of fundamental importance. Two space experiments to test the theories of gravitation now appear feasible. One of these, which involves earth-orbiting gyroscopes, has been undergoing laboratory development since 1963. The other experiment makes use of a sun-orbiting spacecraft. We understand that it is being proposed to NASA by the European Space Research Organization (ESRO) for launch about 1976-1977.

The gyroscope experiment will be able to distinguish between Einstein's theory of general relativity and alternative theories of gravitation to first approximation for weak fields. It will also have the unique ability to detect the gravitational effects produced by matter in motion. This last is especially important in understanding pulsars, black holes, and gravitational waves. The sun orbiter will be able to distinguish between gravitational theories through terms of second approximation. Both experiments can clarify the present uncertainty with regard to the mass quadrupole moment and internal angular momentum of the sun—the ESRO experiment perhaps more directly. This is of great interest in connection with the likelihood of the existence of planetary systems around stars other than the sun.

Technological developments that will follow from the gyroscope experiment include extremely low-drift-rate gyroscopes, long-term maintenance of a liquid helium cryogenic environment in space, and extreme pointing accuracy. The sun orbiter will make use of laser and multiple radio-frequency ranging; an atomic clock will be required aboard. Both experiments are likely to use drag-free vehicles.

Priorities in Gravitational Physics

As part of the BASE missions, the gyroscope experiment should continue to be funded at a level adequate to carry it to launch in 1976. Both the gyroscope and sun-orbiter experiments should be subjected to detailed review as soon as practicable to assure feasibility and reasonable cost. The anticipated costs are thought to be low enough, and the scientific and technological importance great enough, that both experiments should be performed in this decade.

SOLAR-TERRESTRIAL PHYSICS

Through the use of satellites and deep-space probes, the decade of the 1960's culminated several decades of intensive exploration of the near

environment of the earth. After centuries of ground-based geophysical investigations, *in situ* space measurements of the last 12 years have had a revolutionary impact on our understanding of the earth-sun system and interplanetary space out to the orbit of Mars.

The initial exploratory phase of solar-terrestrial physics is essentially complete, and we now face the task of achieving a quantitative, fundamental understanding of the behavior of the gaseous matter and the magnetic and electric fields in the solar system, and their mutual interactions. Impressive results lie within reach of a program that combines experimental and theoretical effort in coordinated, diagnostic experiments.

Solar-terrestrial physics has high intrinsic and extrinsic merit. The proposed research is intrinsically interesting because it bears directly on the questions of the way in which the processes we observe in the solar-terrestrial complex really work. The field has extrinsic merit because the magnetosphere and solar system constitute a plasma physics laboratory that cannot be duplicated on earth and permit direct study of universal processes of basic significance for communications, astrophysics, planetary exploration, plasma physics, and defense.

Because the exploratory phase involved a first look into new regions of space, a strong interplay between theorists and experimentalists was often less important in the past than it will be in the future. Further progress depends much more heavily on cooperative analysis and sharing of data over a wider base of scientific users. Coordinated space research has now been successfully undertaken by a number of scientific groups, and we *urge* that NASA encourage this trend. We *endorse* the recommendation of an earlier Space Science Board study (*Physics of the Earth in Space: A Program of Research 1968-1975*) that the NASA Announcement of Flight Opportunities should indicate the types of coordinated efforts sought, e.g., team efforts, planned data exchange among independent principal investigators, and possible user groups. Such efforts generally are not mutually exclusive, and a given mission may involve experiments in several of the above categories.

Priorities in Solar-Terrestrial Physics

Questions pertaining to the magnetosphere are thought to have highest merit. Some magnetospheric phenomena, such as the aurora, contain fundamental questions of long-standing importance. The answers to these questions are accessible, and we believe that a well-coordinated attack will yield quantitative understanding in the next decade. The BASE missions call for a program of coordinated research on problems of solar-wind energy input to the magnetosphere, internal magneto-

spheric dynamics, and particle-acceleration mechanisms. These new starts include the mother-daughter satellites IMP KK' with orbital apogees of approximately 15 earth radii and a solar-terrestrial probe at 10 million to 15 million km to study the unperturbed solar wind.

High priority is also given to keeping the Atmospheric Explorers C, D, and E on schedule.

The INTERMEDIATE budget program includes the tetrahedral cluster satellites A and B to answer basic questions about the magnetospheric substorm mechanism and collisionless shock phenomena.

We are not so confident that, for equal investments of money and manpower, we would gain as much insight from investigations farther from the earth as we do from near-earth investigations. Therefore, new starts in a Pioneer Heliosphere/Interstellar Mission and a Pioneer Out-of-the-Ecliptic Mission are recommended for priority consideration in the context of the INTERMEDIATE budget planetary program. These two missions, when combined with the solar-interplanetary medium measurements made with the BASE mission solar-terrestrial probe, will provide a complete qualitative description of the nature of solar-wind interaction with the interstellar medium. In the HIGHER budget program, a neutral-point mission and a plasmopause mission are recommended to measure parameters in regions of the magnetosphere that are still poorly defined. In addition, to clarify problems of plasma and auroral particle dynamics in the magnetosphere, a geostationary particle-and-fields satellite is needed in complement with the STP, ESRO, and ATS satellites. Finally, a HIGHER budget Solar-Terrestrial Physics program should include the proposed new Electrodynamic Explorer series of satellites specifically designed to elucidate the couplings between the ionosphere and the magnetosphere.

EARTH SCIENCE

We have considered earth sensing under three general subject headings: earth resources, meteorology and oceanography, and earth physics.

EARTH RESOURCES

Remote sensing can be used in the identification and monitoring of earth resources—agricultural, forest, hydrological, and geological. Remote sensing includes straightforward photographic imaging, of which aerial photography is the most common example, but the term normally implies much greater discrimination, often on an automated basis, than is possible with aerial photographs.

The chief justification of the earth-resources program lies in the expected improvement in environmental monitoring and in resource survey, development, and management. It is accomplished through repetitive multispectral measurements and observations. The requirements are different from those of meteorology and oceanography in that greater spatial resolution and less temporal resolution are required, i.e., the observations must be in greater detail but in most cases at less-frequent time intervals. The benefits of the program lie in the areas of agriculture, forestry, fisheries, hydrology, geology, geography, and ecology. These were studied in considerable detail during a National Academy of Sciences summer study in 1967–1968 (*Useful Applications of Earth-Oriented Satellites*), and the conclusion of that study was that the program is amply justified in terms of expected benefits.

Hydrology concerns the interaction of water with the land, water storage and transport, and water gain and loss. The practical applications are obvious and global. Even the very limited photographs obtained from Gemini and Apollo have convinced hydrologists of the value of earth-orbiting satellites as camera and sensor platforms.

International participation has been widely discussed, but the promise of great economic benefits must be assessed cautiously for the smaller, less technically developed countries. Cost effectiveness is closely linked with geographic size, modern agriculture, highly developed information systems, and a reservoir of trained manpower. Technical assistance is often necessary to expose personnel from participating countries as early as possible to the complexities of problems associated with resource surveys and to familiarize them with the problems of data processing, reduction, and interpretation. The steps taken toward cooperative aircraft sensing programs with Brazil and Mexico as part of the Earth Resources Survey program are highly desirable.

In remote sensing of earth resources, the relative values of airplane and satellite photography vary according to specific objectives. For example, we believe that the airplane is a far better camera platform when small or moderate areas of the earth's surface are to be observed. High resolution is easily obtained, film recovery is convenient, and costs are moderate. In addition, the airplane can carry some sensors, such as side-looking radar, that are not at present practicable in a satellite. In contrast, where global coverage at lower resolution is desired, the satellite has some conspicuous technical advantages and, for frequently repeated global coverage, a large economic advantage. There is quite general agreement with these statements among those who have studied the subject. However, in the intermediate cases between the extremes cited above, neither the technical nor the economic advantages of airplane versus satellite are so clear, and it is not surprising that opinions

should differ as to just where in the scale and kind of operations the advantages shift from one to the other.

It is our opinion that both airplane and satellite imaging systems will be needed, and that it is neither necessary nor wise to attempt to draw the line at this time. A number of the technical questions relating to satellite imaging systems and their application can be answered by the Earth Resources Technology Satellites (ERTS) program. We *urge* continuation of this program, but we emphasize that it is a research and not an operational program, and that practical benefits from it cannot be expected at an early date. To continue to provide data prior to the first ERTS, we *urge* not only that the present NASA program of earth-resources observation from aircraft be continued but also that the data analysis be very much expanded. This will enable users to gain experience in the reduction and utilization of the data for earth-resources application.

Priorities in Earth Resources

Highest priority should be accorded to expanded data analysis in the aircraft surveys for earth resources. These programs should provide evidence on whether useful spectral discrimination is possible. They should also constitute a test of data-processing concepts. Aircraft offer the opportunity to mount intensive programs over limited areas, as opposed to the satellite capability for extensive coverage. The first evidence of economic feasibility for large-scale earth-resources programs is expected to come from the aircraft programs. The Executive Committee of this Study believes that it is inappropriate to extend coverage to worldwide, repetitive surveys before clear evidence has been produced in an intensive program over a relatively small area that the desired discrimination can actually be accomplished.

The approved program for ERTS will provide the first national effort to obtain regular, repetitive resource information. An initial ERTS is scheduled for 1972, to be followed by an identical mission 9 to 12 months later. Estimates of potential economic gains from earth-resources surveys run to tens or hundreds of millions of dollars annually. It is not yet possible to evaluate such estimates, and it should be recognized that the Earth Resources Program does not guarantee economic benefits. However, the Executive Committee believes that the potential gains are worth the risk of the investment and has included NASA's Earth Observation Satellites (EOS) in the program. We *urge* that attention be given to keeping the program realistic and of high quality. This is especially true with respect to data handling and analysis, where the prodigious quantities of data to be generated challenge man's ability to manage and digest them. The recommended program represents an aggressive re-

sponse to the great opportunity that exists in the area, and care must be taken to ensure that the program is not drowned in data before its true value can be demonstrated.

METEOROLOGY

The large-scale circulations of the atmosphere and the oceans are driven by the heat of the sun and modified by the rotation of the earth, and in turn these circulations influence the lives of people in every region and nation. The study of these motions is intrinsically *global* and requires measurements over the entire earth on a repetitive basis; if successful, it promises benefits to the daily lives of mankind everywhere.

Just as the global nature of this problem demands measurements over all the earth, the global impact of the beneficial results depends on international cooperation. Among international programs for peaceful uses of outer space, cooperative studies in meteorology rank among the most successful achievements. The Global Atmospheric Research Program (GARP), an established international program, is the key research and development effort leading to improved understanding of the atmosphere and, it is hoped, to long-range weather prediction. Under GARP, in which the United States is participating, plans have been developed to carry out experiments in the tropical Atlantic in 1973 or 1974 to study the characteristics of cloud clusters and the interaction of various scales of motion in the air and sea, and to conduct a one-year Global Experiment about 1976 that will seek, among other objectives, to obtain a complete set of observations of the earth's atmosphere. This experiment requires the use of space technology—low-altitude (about 1500-km) sun-synchronous satellites and geostationary satellites.

The timing of our space efforts is influenced by these international agreements and by a feeling of urgency to increase our understanding of the atmosphere. The most direct promise of global atmospheric studies, beyond the scientific goal of understanding the important processes, is the well-founded expectation that detailed forecasts of the large-scale features of the atmosphere can be extended to 5 days and conceivably longer. Such forecasts clearly give man an important additional tool in the rational governance of his affairs. More speculative, but perhaps most important, is the feeling that the oceans and the atmosphere, two large systems with vastly different characteristics and coupled at their mutual boundary, govern in their interactions the long-term averages of temperature and precipitation. The ability of satellites to monitor or sample over long periods the sea-surface temperature, some features of ocean circulation, and most features of atmospheric circulations permits us to determine whether we should continue to pur-

sue the goal of forecasting unusual seasons a year or more in advance. The same techniques with which we might successfully predict climate might also be the tools for assessing the impact of man's activities on climate.

Great attention must be given to the requirements of the GARP network for data transmission, processing, and recording. Emphasis must be placed on utilizing new technologies to achieve the maximum output of data in real time for use in numerical experiments and related research.

Priorities in Meteorology

The present meteorological program on both polar-orbiting and geosynchronous satellites has studied several new remote-sensing methods for measuring cloud cover, winds, and temperature in the atmosphere. The future program should refine these techniques as well as investigate methods of humidity sensing, sensing of the small but important changes of physical parameters in the tropics, and extension of measurements to higher levels in the atmosphere.

1. *Earth-Observing Satellites (EOS)*

Among new starts, the Executive Committee *recommends* that sun-synchronous earth-observation satellites (EOS) be launched at a rate of up to one per year to conduct experiments in meteorology, earth resources, and oceanography. This launch rate should provide ample opportunity to develop and test sensors, signatures, and data-handling techniques. These advanced spacecraft are estimated by NASA to cost approximately \$70 million each. The first mission in the BASE missions could concentrate on problems of land and sea surface.

2. *Applications-Technology Satellites (ATS) and (SATS)*

The Committee believes that a rapid response capability to follow fruitful leads is needed as part of the existing ATS program and should be provided by launches of small ATS satellites at a rate of about one every 2 years. Each spacecraft costs approximately \$10 million.

3. *Global Atmospheric Research Program (GARP)*

The Committee further *recommends* that satellite launch schedules and experiment selection be integrated with all phases of GARP, particularly with the Tropical Experiment and the later Global Experiment. We believe that, even if a lower level of activity in the U.S. program is required, the international commitments to the two GARP experiments should be honored.

EARTH PHYSICS AND OCEANOGRAPHY

The Earth Physics Program holds the promise of producing a greatly improved understanding of the basic forces that shape the earth. The program will be accomplished by a variety of spacecraft, some of which require further development before flight. Studies of mantle convection will be furthered by measurements of gravity anomalies, with a resolution approaching 100 km, made by tracking a zero-drag satellite flown at low altitude from another satellite at a higher altitude. Measurements of the separation of points on different continents can provide direct evidence of continental drift if these measurements have the accuracy inherent in laser ranging or very long-baseline interferometry. Although much development work is required, accuracies of 2–19 cm appear to be within reach. Such measurements may also be able to follow the “wobble” of the earth’s axis. The value of this knowledge in practical terms may be far in the future, but its scientific merit is high. The BASE missions include both supporting research and technology and the satellite measurement program.

The development of high-precision satellite altimetry is also fundamental to work in oceanography. Because tidal forces, winds, and barometric pressure vary from point to point over the oceans, the distance from center-of-earth to sea surface is also variable. Only small patches of ocean can be surveyed each day from ships, planes, and submarines. A polar-orbiting satellite altimeter would give a semidiurnal, worldwide, coarse-grained topographic map of the sea surface and, in principle, could permit the measurement of: (1) tides, waves, and, perhaps, tsunamis; (2) flow patterns of ocean currents; (3) atmospheric pressure and winds over the oceans; and (4) volume of sea ice and its movements.

GEOS-C, planned for 1972, is to orbit at a height of 1000 km and to carry an altimeter capable of achieving an accuracy of 2 m, sufficient to provide usefully detailed information about the shape of the geoid. Improving the accuracy to ± 1 m would permit detection of tides over continental shelves, sea-surface elevations associated with western boundary currents such as the Gulf Stream and Kuroshio, and the rise and fall associated with storm surges. Late in the 1970’s, a resolution of 10 cm may be achievable and would permit observations of the general oceanic circulation. Detailed day-by-day observations would increase our understanding of energy transport across the air–ocean interface and improve the accuracy of weather prediction by numerical forecasting. To determine ocean-wave statistics relevant to electromagnetic reflection, much preliminary research in perfecting radar altimetry is needed, both in the laboratory and the field.

Priorities in Earth Physics and Oceanography

For the BASE missions, high priority is assigned to the drag-free satellite tracked from another satellite and to the development of laser ranging and very long-baseline interferometry for use in spacecraft. These programs are expected to make major contributions to understanding of the movements of continents relative to one another and to the forces responsible for this motion. Other elements of the earth-physics program can be accomplished in Small Applications Technology Satellites (SATS), also recommended for the BASE missions. Earth Observatory Satellites (EOS) in the BASE missions will contribute to oceanography through observations of sea-state and sea-surface temperature. At the INTERMEDIATE budget level, development of a high-precision altimeter (10–50-cm accuracy) should be pursued.

LIFE SCIENCES

Life sciences pertinent to the space program can be discussed in terms of exobiology, biomedicine, and space biology. Exobiology, in its broadest definition, involves not only the search for evidence of past or present extraterrestrial life but also for indications of nonbiological chemical evolution that could support or clarify our present ideas about the origin of life and the possibility that terrestrial life might survive on other planets. This field has almost universally caught the imagination of scientists and the public at large. Group after group and report after report have stated that the discovery of life outside the earth would likely rank among the greatest scientific discoveries of the century. The questions are fundamental; the interest and excitement are high. The study of exobiology—particularly if life is found elsewhere in the universe—will have a profound impact.

Biomedicine includes the basic physiological and psychological information necessary to qualify man for spaceflight. The Life Sciences Study (*Life Sciences in Space*) states “that unless [research in these two areas] can be done, all other missions depending upon man in space must fail or be severely handicapped.” Biomedicine also includes a variety of clinical tests designed to define and be predictive of the astronaut’s state of health. It further involves the unknown and unexplored areas of human behavior in small groups isolated for long periods of time. There is no question but that the biomedical studies should include basic research. On the assumption that manned spaceflight will continue, we *urge* that a much stronger and more broadly based program of research in the physiology and psychology of man in space be planned and executed.

The third area, space biology, deals with basic research into the effects of the space environment on living organisms in order to increase our understanding of fundamental biological mechanisms. Some prominent topics in this category are: (1) the use of satellites and aircraft to study earth ecology and to track animals in order to learn how animals orient themselves and navigate with pinpoint accuracy over thousands of miles; (2) basic behavioral biology, including biological rhythms, in which low *g*, absence of terrestrial influences, and other factors of spaceflight are considered; (3) gravitational biology, which includes both morphological and physiological investigations designed to gain an understanding of the role of gravity in animal and plant development, form, and function. Weightlessness, a phenomenon unique to the space environment, is a newly accessible variable with which to study basic biological properties. Investigations include the role of gravity in cellular processes, in morphogenesis, and in the function of specific tissues and organ systems. In animals these include orientation systems, the circulatory system, weight-bearing systems, and metabolic processes; in plants, orientation mechanisms, metabolic processes, reproductive processes, and weight-bearing tissues; and (4) determination of the biological effects of radiations found in the space environment, such as high-energy, high-*Z* (HZE) particles and solar-flare emissions.

Priorities in Life Sciences *

The field of exobiology will have a continuing need for supporting research and technology (SR&T). The Viking lander on Mars with its life-detection payload is an ongoing project that must have continuous earth-based research support. The field of biomedicine involved in qualifying man for spaceflight and in estimating the human hazards of such flight will also require extensive SR&T funds. Some preliminary experiments are projected in Skylab. If man is to continue his venture into space, it is imperative that he be able to function well, not only in terms

* Because of the administrative fragmentation of the life sciences within NASA at the time of this Study (August 1970), some words of explanation on funding divisions are necessary to clarify the priority statements that follow. The Viking Project is supported through the OSSA Planetary Programs Office; exobiology as a field of research is funded by the Bioscience Office. Similarly, manned spaceflights and the experiments carried on them are funded by the Office of Manned Space Flight, but additional research is carried on by both OSSA Bioscience and the Office of Advanced Research and Technology. Earth-observation programs as a whole are funded by the OSSA Space Applications Office, while certain related aspects of basic research are supported through OSSA Bioscience. We address ourselves to the NASA-wide program in life sciences, but our funding recommendations refer primarily to the OSSA Bioscience budget.

of his own well-being, but also in terms of the success of any mission of which he is part. We believe, however, that it is necessary to take a broad view of the type of research needed in this field, and thus we find that SR&T funds are necessary for the applied research as well as basic biological research requisite for any advances in medicine.

Earth ecology today emerges as a predominant area for study. In this field, which encompasses the study of the earth and its biosphere, there is some urgency to the studies because irreversible changes are taking place. These programs are a part of the Earth Observations program. It is the belief of many biologists that satellites with remote sensors will be able to supply information needed for the formulation of better models of ecosystems and for the testing of such models. The severe ecological problems facing us give studies added importance in the coming decade. In general this problem will be attacked by the Earth Resources and Application program, which will supply the data. The ecological part of the program will have to be financed by SR&T funds.

The field of gravitational biology includes many problems that are intrinsically worthwhile, and these should be looked into in the proper vehicle. Although the report of the Santa Cruz study (*Space Biology*) was critical of some of the previous experiments carried out in biosatellites, it also looked forward to the day when a Skylab or shuttle and space station would be available in which experiments on low *g* could be carried out free of the confounding variables of, for example, vibration and acceleration, and in which the experiments could be attended in flight by the investigator. Until such a vehicle is ready, however, SR&T funds will be vital to support the research required to develop a flight package and to establish the proper baseline for comparison and understanding of the later flight experiments.

1. Level of Effort

We *recommend* that in the BASE missions NASA not only continue but expand its SR&T funding in exobiology, biomedicine, and space biology. The present level of OSSA Bioscience funding is \$12.9 million (including funds for planetary quarantine). We *recommend* that this be increased to \$18 million.

2. Flight Definition and Improved Biosatellite

We *recommend* that in an INTERMEDIATE budget program life-sciences experiments be carried on ongoing missions, particularly on manned flights to take advantage of the presence of man for the manipulation of delicate biological materials. In a HIGHER budget program we envisage development of an improved biosatellite. Its priority, however, is relatively low.

II REPORTS OF THE WORKING GROUPS

3 Planetary Exploration

The coming decade holds the promise of a preliminary reconnaissance of the solar system we inhabit, of a close-up examination of the marvelously diversified collection of worlds and debris that occupies our neighborhood in space. These planets, satellites, comets, and asteroids are the only objects that we have any chance of visiting in the foreseeable future, and there is a considerable difference between remote and on-the-spot investigations.

The first few steps in planetary exploration by space vehicles have so far revealed Venus as a searing hot world, with a deep atmosphere composed mainly of carbon dioxide, a dense cloud layer of unknown extent and composition, and hints of the breakdown of water on a massive scale during its history, and Mars as a cratered, eroded planet, varying greatly from place to place, boasting vast 10-mile-high plateaus, dry-ice polar caps, fierce winds, and occasional giant dust storms.

The principal scientific objectives of planetary exploration have long been recognized as including (1) determination of the mode of origin and course of development of the solar system, (2) clarification of the origins and evolution of life, and (3) an improved understanding of a range of fundamental scientific problems first formulated for the earth and having conceivable practical implications.

The first two of these objectives speak to a desire, felt by scientists and nonscientists alike, to discover where we are in the universe, how we got here, and where we are likely to be headed—a quest for a kind

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of cosmic perspective for mankind. We are the product of a long evolutionary history which traces back to simpler forms of life, to the chemical events that led to the origin of life on earth, to the very early history of our planet, and to the formation of the solar system probably from an interstellar cloud of gas and dust. We seek to understand the generality of these events. Arrayed before us are a variety of objects that have relevant information to give us. The Jovian planets and their satellites, the comets, and the asteroids contain chemical information on the early solar system. Mercury, Mars, and the satellites of the planets promise clues on the final stages of formation and the subsequent surface evolution of planets. Venus may tell us about the outgassing history of planets and about possible catastrophic developments of planets rather like the earth—whether man-made increases in the amount of carbon dioxide or water in our atmosphere could lead to higher temperatures, more such gases in the air, higher temperatures yet, or other such phenomena, resulting in the runaway greenhouse effect. And Mars, because it has an atmosphere and some crustal development, and because its surface alone is accessible in the 1970's to an appropriate lander, is the only plausible locale for an early search for extraterrestrial life.

The evolutions of planets and life are intimately connected. The environment strongly determines the likelihood of the origin and continuance of life, while life, once present, reacts to influence the environment. A proper approach to exobiology encompasses an entire continuum of possibilities that ranges from a lifeless planet with no signs of past life and no prebiological organic chemicals to an inhabited planet. Even a lifeless planet is of great interest: it provides a control, unaltered by biology, with which the earth can be compared.

Planetary exploration permits us to see our planet and ourselves in a new light. In many respects we are like linguists on an isolated island where only one language is spoken. We can construct general theories of language, but we have only one example to examine. It is unlikely that our understanding of language will have the generality that a mature science of human linguistics requires. There are many branches of science in which our knowledge is similarly parochial, restricted to a single example among a vast multitude of possible cases. Only by examining the range of cases available elsewhere can we devise a broad and general science. This is true for meteorology, for which other planets serve as natural control experiments for the influence on atmospheric circulation and of rotational forces, oceans, topography, solar and infrared radiation, and radiative relaxation times. It is true for geology, for which other planets provide natural control experiments on the influence of interior motions on continental drift and the formation of folded mountains; on

chemical differentiation and core formation; on the roles of planetary spin and a nearby moon in the origin of geomagnetism; on erosion and weathering in the absence of liquid water; indeed, on the entire question of the comparative anatomy and evolution of planetary surfaces and interiors. And it is true for biology, which has had only a single example of life to study, because all earth organisms use fundamentally the same biochemicals and the same genetic code.

The technology and celestial mechanics opportunities for exploration of much of the solar system are ready, for the first time, in the 1970's. Exploratory missions to the deep atmosphere of Venus, the surface of Mars, past Mercury and five outer planets, 31 satellites, and comets and asteroids never before examined at close range carry enormous prospects for surprising discoveries and major revisions of present views in planetary science. After such preliminary reconnaissance, there will be a very large field for detailed development in subsequent decades.

By providing an extraterrestrial perspective, planetary exploration will make fundamental contributions to earth sciences, meteorology, biology, and a range of other disciplines. By providing a framework for consideration of the origin of the solar system and the origin of life, planetary exploration is likely to bring, even in a moderately short time scale, a new view of nature. The frontier technologies used on planetary missions tend to have broad application. For example, the STAR (Self-Testing and Repair) computer being developed for the outer-planet Grand Tour has conceivable application to automatically maintained communications and meteorological satellites in earth orbit, to automatic stations in the abyssal depths, and to commercial, industrial, and household computer systems of the next decade.

Because a vigorous program of planetary exploration is under way in the Soviet Union and, to a lesser extent, in other countries, there are important immediate opportunities for international cooperation, ranging from complementary exploration programs, experimental interaction, and data sharing to joint payloads and international investigator teams on the same experiment.

There is no doubt that the general exploration of the solar system and the search for life elsewhere are ventures, unlike others of equal scientific merit, that find strong responses in popular interest and strike chords of philosophical excitement infrequently sounded by the man in the street. The perspective of our planet and our species in a vast and unknown universe tends to mature our thinking about mundane problems. Many of the leaders of the ecological action movement in the United States have been stirred to activity by photographs of the earth taken from space, which show a tiny, delicate, and fragile world, exquisitely sensitive to the

depredations of man. The detailed examples of other planets, for example, of Venus as a world where a kind of runaway atmospheric pollution has occurred or of Jupiter as a world on which the first steps to the origin of life are today occurring, can only increase the social awareness of who we are, how we got here, and what possibilities there are for controlling where we are going.

While there are several areas of ground-based and near-earth planetary observation that well deserve vigorous exploitation (for example, interferometric infrared spectroscopy), many of the most pressing problems can be answered only by *in situ* experiments. Some of these experiments can be performed by relatively inexpensive systems (e.g., the Explorer concept for Venus atmosphere entry experiments); others, such as many geological and biological investigations of planetary surfaces, can be approached only through larger systems. We are agreed that the optimum approach is a mixed strategy in which both general exploratory ventures and observations designed to answer specific questions are accomplished. But there are time constraints, for Mars because of the urgency of performing biological experiments while the likelihood of microbial contamination is still small, for Venus because of the desirability of keeping together teams that have already worked years on the entry probe concept, and for the outer planets because even two-planet swingby mission opportunities do not recur for almost two decades.

RECOMMENDED PROGRAM

We recommend a program for planetary exploration that follows the general guidelines presented in the 1968 Space Science Board study, *Planetary Exploration 1968-1975*. No single goal is adopted. Rather we endorse a program that emphasizes a broad range of scientific disciplines. Whenever possible, the exploration program is designed to acquire preliminary data for all planets. Attention is focused on individual objects only if the intrinsic and technological factors warrant this attention. We believe that close scrutiny of our nearest planetary neighbors, Mars and Venus, is warranted at this time.

An orderly program for exploration begins with extensive ground-based study, continues with remote observations in a flyby mode, and leads perhaps to orbiting or atmospheric probe missions. For planets with identifiable surfaces this strategy culminates with landed payloads. All the planets have been extensively studied by conventional astronomical means. We have flown by Venus and Mars, and flyby missions to Mercury and Jupiter are at an advanced stage of development. The Grand Tour

offers an opportunity to extend the strategy to Saturn, Uranus, Neptune, and Pluto. Mariners 8 and 9 will orbit Mars and begin a more detailed study of that planet. Viking will continue the Mars program, landing an ambitious scientific payload on the surface. Planetary Explorer missions to Venus will begin a more concentrated strategy for Venus, and we envisage atmospheric probes and orbiters to Jupiter by the early 1980's that will build on results derived from the earlier flybys. We also contemplate an orbiting spacecraft to Mercury, perhaps using solar-electric propulsion. The case for a Mercury orbiter depends, however, in large measure on results obtained in the Mariner flyby mission.

The program discussed above is directed toward exploration of planets and satellites. A balanced program should also include smaller bodies—comets and asteroids. Unfortunately, planning for such missions is at a preliminary stage. We endorse the scientific merits of these missions and urge more detailed study, specifically of possible rendezvous or docking missions.

Viking is an ongoing approved program strongly endorsed by this Working Group. The 1975 mission appeals to several areas of planetary science, although certainly it finds strongest support from bioscientists. If the budget permits we would recommend a follow-up Viking program. The direction of the program would be strongly influenced by the results obtained in 1976. If the biological elements of the 1975 mission give positive signals, we would contemplate extensive biological bias in future missions. Otherwise, follow-up missions should emphasize geophysical exploration. We believe that the surface exploration program for Mars should be closely coordinated with any lunar automated program. We specifically recognize the possibility of a rover with both lunar and Martian capability.

Planetary Explorer missions to Venus are a key ingredient of the planetary program. We recommend a launch schedule involving two probes in 1975, followed by an orbiter in 1976, and follow-up missions whose character should be strongly influenced by results obtained in the early flights. We consider low-cost surface science to be of high priority. The Planetary Explorer shows promise of providing this option. It should be carefully studied, and we would warmly endorse it if the cost is in line with the Planetary Explorer concept. We would not endorse any Venus missions at this time if the cost were comparable with projected costs for the Viking missions.

During the period 1976–1980, opportunities will exist for missions to planets beyond Jupiter that can use Jupiter to shorten greatly the transit times that would be required for direct trips to those outer planets. Such missions are referred to as the Grand Tour. In the case of Uranus, the

time from earth is shortened from about 7 years to 4 years, and in the case of Neptune, from 13 years to 7 years if advantage is taken of the swingby opportunity. Thus, with launches toward the end of this decade, data from flybys of the outer planets would become available during the period 1979–1986.

The next opportunity for a swingby mission to another planet—even to Saturn—will not occur until the early 1990's. Thus, if the present opportunity is lost we must postpone our entire program to explore the other planets by 15 years. On the other hand, if we use this opportunity there will be time between the flybys in the mid-1980's and the next opportunities in the 1990's to develop follow-on missions. In fact, we visualize the possibility of launching direct-entry-probe missions in the 1990's to Saturn, Uranus, and Neptune in swingby (high-velocity) modes and direct trips to these planets using new technology (nuclear-electric propulsion) whose purpose will be to place spacecraft in orbit around those planets. It is clear that by launching Grand Tour missions in the late 1970's we will be able to begin a systematic program to explore the outer planets which proceeds in two phases—flyby first and probe second.

It is also clear that it will be possible to carry out very useful observations beyond Saturn on these missions. In addition to conventional imaging of the planets and their satellites at high resolution over periods of many days and months it will be possible to measure the composition of their atmospheres in earth occultation (radio) and solar occultation (ultraviolet) experiments. Furthermore, a very impressive array of direct *in situ* astrophysical and solar physical measurements will be possible during these flights, some of which will leave the solar system and some of which will go out of the plane of the ecliptic into high heliocentric latitudes. Sampling of low-energy cosmic rays beyond the solar cosmic-ray cavity will enable us for the first time to determine the density of these particles in the galaxy, without the possibility of confusion from solar particles. Most cosmic-ray energies are in the low-energy part of the spectrum. The spacecraft will pass through the shock where the solar wind impacts the interstellar medium (the heliopause). The particle fluxes, energy spectrum, and magnetic fields can be sampled in this region. Beyond it, measurement of low-energy H^+ and H densities will reveal whether the sun is imbedded in an $H\text{ I}$ or $H\text{ II}$ region. Other observations will elucidate processes by which $H\text{ I}$ regions are heated, test models of galactic winds, and add to our understanding of the steps to star formation. It is remarkable that during the opportunity of the late 1970's probes will leave Jupiter in the direction of the apex where the heliopause is closest to the sun (5–20 AU). Later in the century they will tend to travel in directions antiparallel to the direction of solar motion where the boundary may be some 60 AU distant.

The out-of-the-ecliptic swingby mission offers a great opportunity to break new ground in solar physics. Examination of the sector pattern of the solar magnetic field at high heliocentric latitudes and the pattern of energetic particles will shed light on theories of stellar rotation. These, in turn, are related to questions of the origin of planetary systems.

In 1975, the Pioneer missions will reveal something about the degree of the danger to spacecraft from debris in the equatorial plane near Jupiter. If necessary on the basis of this information, the Grand Tour spacecraft can be retargeted to pass farther from Jupiter, increasing the trip time to the outer planets. Or, if it is deemed improbable that the spacecraft can get past Jupiter at all in the proper orbit, the entire mission can be altered (with a 3.5-year lead time) to permit the spacecraft to enter the Jovian atmosphere or to go into orbit in a plane other than equatorial.

We conclude that first-order questions in planetary, solar, and astrophysical science will be answerable by Grand Tour experiments, and that the uniqueness of this opportunity is so great that it should not be missed. It is also apparent that a mission of high reliability is very desirable, because much of the interesting astrophysical information may not be obtained until the probe has traveled 10–20 AU. These considerations lead us to endorse the high reliability promised by the Thermoelectric Outer Planet Spacecraft (TOPS).

The key ingredients of the recommended program are summarized in Table 1.

GUIDELINES FOR MANAGEMENT OF PLANETARY PROGRAM

The principal consideration guiding the management of the planetary program should be the need for balance. By balance, we refer to our mixed strategy in which inner planets are explored systematically in depth while a first look by remote sensing from flyby missions prepares us for similar *in situ* probing of the outer planets. To maintain such balance it will be necessary to protect small projects such as Planetary Explorers. These missions should not be pushed out of the program by larger projects of a different type, such as the Grand Tour. Thus a fallback position from a program containing Grand Tour and Planetary Explorer would replace Grand Tour with either a less costly Grand Tour option (modified Pioneer, for example) or at a lower level with Pioneer missions to Jupiter and Saturn. Planetary Explorer should be preserved in such a retrenchment. We strongly urge that NASA use the criteria discussed here in setting up and managing its programs.

TABLE 1 Recommended Planetary Program (Dates Indicated Are Launch Years)^a

Planet							
Mode	Mars						
	Venus						
	Mercury						
	Jupiter						
	Saturn						
	Uranus						
	Neptune						
	Pluto						
Flyby	M 1973(1)	M 1973(1)	P 1972(1)	GT 1977(2)	GT 1979(2)	GT 1979(2)	GT 1977(2)
			P 1973(1)				
			GT 1975(1)				
			GT 1977(2)				
			GT 1979(2)				
Orbiter	M 1971(2)	PE 1976(1)	SE 1982	GT 1981			
Atmosphere probe	PE 1979(1)	PE 1975(2)		GT 1980, 1984			
Lander	V 1972(2)	PE 1980(2)					
	V 1977(1)						
	V 1979(1)						
Surface rover	1981(1)						

^a M, Mariner class; PE, Planetary Explorer; V, Viking; SE, solar electric; P, Pioneer; GT, Grand Tour (tops).

We also consider possible fallback positions in the event that Viking were eliminated from the program for budgetary reasons. If this happened, our strategy for the inner planets would be unbalanced: there would be no provision in the program either for *in situ* atmospheric probing or for landed science on Mars. For atmospheric studies the deficiencies might be rectified by extending the Planetary Explorer concept to Mars. It should be possible to prepare surface meteorological and geophysical instruments for hard and rough landings. Thus surface values of wind, temperature, pressure, water vapor, surface composition, and seismic data could be obtained by Explorer-type landers. Biological investigations, however, would probably not be possible.

There is reason to believe that the most important information needed to understand the general circulation of the atmosphere cannot be obtained from surface data. Nor is it likely that this information can be obtained from orbital imaging of drifting clouds; first, because clouds are few on Mars and their elevations will be difficult to determine, and, second, because the same technique applied to the earth's atmosphere, which has many more clouds, has been notably less successful than initially advertised. This leads us to the concept of helium-inflated, constant-level balloons launched from a lander and tracked by an orbiter. At first glance it would seem unfeasible to fly balloons in a low-pressure atmosphere, but some of the difficulty is canceled by the high molecular weight of the Martian atmosphere as compared with the earth's. Added to this is the high state of development of balloon technology which has recently culminated in a successful flight at 1 mbar in the earth's atmosphere with a heavy payload. Preliminary calculations indicate that 10–20-m-diameter balloons can be flown at the 1–3-mbar levels of the Martian atmosphere carrying useful payloads weighing several ounces to 1 lb. To establish feasibility, engineering study is warranted.

The geological and geophysical experiments can also be designed into a rough lander system. Two important areas of investigation are determination of the major elements composing the surface and the seismicity and structure of the planet. Such experiments have been successfully flown to the moon. For surface composition, candidate experiments (alpha scattering, x-ray fluorescence, neutron-capture gamma-ray spectrometer) can be lightweight and simple. Miniaturized seismometers that can withstand great shocks and operate under severe temperature conditions have been developed. The three-axis Viking seismometer weighs less than 2 lb.

Data from these modest experiments would be significant not only for understanding the planet but also for planning future exploration strategy.

MISSIONS IN ORDER OF PRIORITY

Following these guidelines we present planetary-exploration projects in order of priority. We exclude the baseline approved program, including Viking 1975, from consideration here.

First priority goes to Planetary Explorer missions to Venus and Mars and to some form of remote probing of the outer planets. We recommend that the sequence of Explorer probes and orbiters for Venus and the Martian orbiters described above under Recommended Program be preserved down to the lowest budget level that can accommodate them. If the Viking 1975 mission is canceled for budgetary reasons, we recommend addition of Martian atmospheric probes to this program. No new start on any other planetary program should be allowed to compromise these missions. We recommend that exploration of the outer planets proceed at the highest level consonant with preserving this inner-planet program. This means the full Grand Tour mixture, if possible technically and fiscally. The substitution of a combination of Jupiter orbiter and Saturn flyby of the Pioneer class in the late 1970's, planned on the basis of Pioneer F and G results, is an ultimate fallback option at low budget levels.

Next in order of priority, assuming that the Grand Tour is approved, is the beginning of the exploration of Jupiter in depth as represented by the large probe orbiter missions described in our recommended program. This is followed by the beginning of surface science and atmospheric circulation studies for Venus, the Martian Geoscience Viking 1977 lander, the extensive Viking follow-on program culminating in the Martian rover as determined by the results obtained by Viking 1975, the small-body rendezvous missions, and the Mercury orbiter. If Viking 1975 produces positive signals for the existence of life on Mars, the Viking follow-on missions would rise to the highest level of priority.

4 Lunar Exploration

Until the first samples were returned from the moon by the Apollo 11 astronauts, there was little prospect that scientists would ever acquire more than scraps of direct evidence about the first billion years of earth or solar-system history. Although the earth and the solar system are about 4.6 billion years old, the oldest earth rocks that have been accurately dated are only 3.4 billion to 3.6 billion years old. On the moon, even young-appearing features such as the maria are 3.4 billion to 3.6 billion years old, and there is evidence that the older rocks of the lunar highlands may have ages approaching 4.6 billion years. Studies to date indicate the existence on the moon of a diversity of very old rocks representing this first billion years of planetary history. These rocks may reveal much about the early history of the earth and other terrestrial planets if they can be put in the right chemical context and time sequence. The improved model of earth's early differentiation which should emerge from lunar studies would be of great scientific interest and, possibly, of social interest as well. Insofar as it bears on the origin and boundaries of the early geochemical and metallogenic provinces of the earth, such understanding carries a significant potential for practical applications.

The historical record of our planetary neighbor appears to be rich during the period for which such a record is missing for the earth. This has profound implications for our understanding of both the earth and the solar system and perhaps for the origin and distribution of ore deposits which are concentrated in the older rocks on earth. As an important side benefit, the results of lunar studies, together with those from sea-floor

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geophysics, are playing a major role in a conceptual revolution of far-reaching impact in the earth sciences and stimulating fruitful new interactions among the earth sciences and related aspects of chemistry, physics, and biology.

The highly significant data on lunar chemistry and chronology obtained from samples returned are only part of the scientifically significant results of the Apollo missions to date. Others include the unexpected discovery of a magnetic field and the curiously persistent—and unexplained—seismic signals. These suggest the likelihood of new and important surprises from future studies, as well as directions these studies should take. Although, therefore, the Apollo program was undertaken for primarily nonscientific reasons, and the goal of placing man on the moon and returning him safely to earth has been accomplished, it is the considered judgment of this Working Group that the scientific potential of continued lunar exploration justifies the completion of as much as possible of the proposed Apollo program and a follow-on automated program. We are just beginning to understand the nature of the lunar processes, and more data on ages, chemistry, geology, and physical properties of the moon are necessary if we are to piece together a coherent picture. The Apollo program is now aimed at establishing some primary scientific data points. Such points are essential in attempting to calibrate information already gained and to be gained from telescopic study, orbital photography, and eventual automated surface studies in such a way as to reconstruct the history of lunar differentiation and, from it, that of the early earth and primitive terrestrial planets. This differentiation history can be obtained *only* by the study of documented samples from a sufficient number of lunar-surface sites and traverses and by establishing an adequate geophysical data net.

The long-range goals of lunar science may be summarized as follows:

1. *To use the moon as a window to the early history of the earth-moon system.* It may be possible to reconstruct the missing first billion years of earth history by studying the oldest lunar rocks. It is essential, therefore, that suitable highland landing sites and traverses be included in the remaining Apollo program, with sample returns for geochemical and age analysis.

2. *To use the moon as a focus for study of the early evolution of the solar system, particularly the inner planets.* How long did it take for a terrestrial planet to condense from the dust of the original solar nebula, and what was the rate of meteoritic infall during the final stages of condensation? When and by what processes did the mare surface materials attain their peculiar chemical signature and physical properties,

and how does this bear on early planetary evolution? These questions can probably be answered by geological, geochemical, and geophysical analyses, for a precise time framework determined by nuclear age-dating. Constraints that will permit a narrowing of choices among competing models of lunar origin should emerge from such analyses.

3. *To use the moon as a yardstick to compare the results of surface processes and physical evolution on a small planet (the moon) without a permanent atmosphere, and probably without life, with the surface processes that have affected the earth.* What produced the thick layer of fine debris that blankets much of the lunar surface? What is its history and rate of accumulation, and how does it differ from place to place? How much of this debris is of meteoritic origin, and in what ways has the meteoritic component affected its erosion and structure? To what extent are lunar surface-temperature variation and solar radiation involved in lunar weathering and surface chemistry? Is an evolutionary record of the sun's emissions to be found in the fine debris? Geochemistry combined with electron microscopy of experimental and earth analogues of morphological features observed may give the answers—or at least focus discussion on possible alternatives.

A collateral goal is the further assessment of the small but still existent possibility that life may have existed on the moon in the distant past or that pre vital organic molecules may have been brought there by meteorites. The fact that no microorganisms or their products or resting stages were found in samples returned from the first two lunar landing sites makes it highly unlikely that indigenous life now exists anywhere on the moon. But this does not say that life could never have existed on the moon (even though that also seems improbable). The vesicular state of the mare lavas implies that they contained quantities of matter volatile at local temperatures and pressures. If small amounts of water were among such volatiles and persisted for any length of time, life or its organic antecedents might conceivably have originated at such sites and might, therefore, be preserved as relics in lunar sediments. It would be unthinkable, as additional samples became available, not to pursue such remote but gripping possibilities in an attentive and critical way or to ignore the possibility of finding uncontaminated organic materials of meteoritic origin.

4. *To study how and when the moon became segregated into different chemical and physical provinces as a model of how such differentiation might proceed on a small terrestrial planet like the moon, Mars, or Mercury.* Much can be inferred about differentiation history from surface morphology, which provides clues to function and origin in planetary and earth science, as it does in biology and astronomy. But we need much

better ground control and chronological information than is now available to sharpen the resolving power of such morphological criteria. Important aspects of lunar differentiation history include its gravitational asymmetry, its peculiar seismic behavior, and its magnetic and thermal properties. Networks of lunar geophysical stations, with data-receiving and data-analysis systems, are essential to elucidate these aspects.

5. *To use the moon as a testing ground and staging area for ultimate expeditions to other parts of the planetary system, as well as for new kinds of studies on or from the moon itself.* Systems designed to investigate other planets can be developed, tested, and perfected in lunar application while adding to our growing knowledge of the moon and solar system. The advantages to astronomy, for example, of a lunar base in, say, the Mare Orientale or on the far side of the moon deserve careful consideration.

The kinds of research that must be performed to achieve the above goals fall into three main categories: the sequence and time scale of events in lunar evolution and their correlation with the evolution of the earth and planetary system; the structure, composition, and processes of the lunar interior; and the geochemical, petrologic, and geomorphic characteristics of the lunar surface and the processes that have determined and modified them. The organic geochemistry of the moon, extending from the simplest organic molecules through a sequence conceivably extending to biological forms, is a collateral matter which continues to be of interest.

The observations needed to gain an initial understanding of these matters are by no means certain, but lower limits can be set. The number of sampling sites required is primarily a function of the number of major lunar terrain features that must be studied in order to evaluate and calibrate surface morphology as a clue to lunar differentiation history. A minimum of 10 to 15 landings is considered desirable by this Working Group and by previous studies (see, for example, *Lunar Exploration: Strategy for Research 1969-1975*). Because of the cancellation of Apollo 20 and the abort of Apollo 13, only six such landings are now planned.* Any further reduction would jeopardize attainment of the scientific goals of the Apollo program.

RECOMMENDED PROGRAM AND ITS MANAGEMENT

The fields of study that must be supported in order to complete the scientific aspects of the Apollo missions are geochronology, geochemistry,

* Since the study, two additional Apollo missions have been canceled.

and petrology; regional geology and terrain analysis; and geophysics. Their applications to the moon and some of their interactions with other fields are described briefly below.

Geochronology

The moon is the most accessible body in our planetary system that could provide an ordered record of earliest events. In particular, the correlation of lunar and terrestrial geochronology is crucial to an explanation of the earth-moon dynamic system.

The lunar rocks from mare sites, according to radiometric age-dating, are younger than the lunar fines from mare sites studied and the planetary system as a whole. In order to place lunar geochronology in proper context, similar measurements need to be extended to older highland sites, to younger impact craters and volcanic features that postdate the maria, and to material thrown out by volcanism or impact from deep within the moon.

Landing sites proposed for the six remaining Apollo flights take these considerations into account. Older rocks are expected from the Tycho, Descartes, or Hadley-Apennine sites, and the youngest magmatic rocks from the Marius Hills. Material from deep within the moon may be obtained from the ejecta of the larger impact features, from maar craters, or from the central peaks of impact craters such as Copernicus. These and other sites will also yield direct information about the range in age and nature of magmatic events.

Geochronology has application to all other problems concerning the moon and its environment. The presence of relatively large amounts of noble gases in the lunar soil, implanted by the solar wind, offers an opportunity to investigate solar chemical evolution by studying their concentration in lunar surface samples of different ages. The flux of impacting solid particles from extralunar sources can also be investigated through study of the microcratering phenomena observed on the surfaces of dated rock fragments.

Geochemistry and Petrology

Geochemical, mineralogical, and petrological studies of lunar rocks and fines characterize these materials with respect to their elemental abundances, identify the phase assemblages in which these elements appear, and relate the data to lunar petrological history. These findings, compared with the corresponding data for the earth and meteorites, lead to a chemical history of the planetary system, including earth, and provide

the necessary context for geochronological and geophysical interpretations of the moon.

Lunar geochemical data presently available, based on the study of very small samples from two mare sites, have revealed significant differences in chemical abundances as compared with terrestrial and meteoritic material. It is doubtful, however, that these data are representative of the moon as a whole, and the pattern will not be reasonably clear without a representative sampling of the lunar crust.

Igneous rocks found at the Apollo 11 and 12 sites have crystallized under strongly reducing conditions, which differ from conditions for corresponding terrestrial rocks. Is this condition peculiar to the rocks of the maria, or is it a characteristic of lunar chemical processes in general? Is the almost completely anhydrous nature of the rocks from these two maria a local or a general feature of the lunar chemical environment? These are general questions that relate to the moon as a whole and to its relations with earth and other planetary bodies. Such questions can be answered only by representative sampling of the lunar crust and interior.

Regional Geology and Terrain Analysis

Morphological analysis of the various visible surface features of the moon (volcanic impact, constructional, strain, erosion), when correlated with surface geochemistry, petrology, and geochronology at the landing sites, provides the key to extension of lunar differentiation history wherever adequate orbital photography is available. A large amount of such analysis has already been attempted in the lunar surface mapping program of the U.S. Geological Survey, but no on-site control was available before Apollo 11. Rover traverses and further sample analysis are needed to check the validity of such mapping, to test its generalizations, and to lay the groundwork for the extension of such interpretation to the far side of the moon. The basis for interpretation is founded on well-established principles of terrestrial landform analysis.

Geophysics

Lunar geophysics is concerned with physical properties, such as thermal regime, gravitational field, seismicity, magnetism, and paleomagnetism and with the density and state of lunar matter as functions of both radial and lateral dimensions.

Knowledge of the internal temperature and heat flow of a planet yields important information, setting limits on the state and strength of the

rocks as a function of depth and placing restraints on permissible dynamic processes. Internal temperature is largely determined by radiogenic heating; ratios of the radioactive elements K, U, and Th are sensitive indicators of geochemical fractionation that yield information on the bulk composition of the body. The thermal characterization of the highlands, of the maria, and of the lunar far side are of special interest as lateral variations in heat flow afford evidence on the gross structure of the lunar surface. Heat-flow measurements in the Apollo program are limited to shallow depths, hence they are subject to relatively large local variations in homogeneity of the lunar material and to local variations in the surface temperature. Such difficulties can be partially overcome by increasing the number of sites examined.

The study of lunar gravity must be in sufficient detail to calculate the lunar figure of gravity and include surveys across such mascons as may be found on the far side. Such preliminary surveys are best effected by a close orbiter, using a satellite-to-satellite tracking system for communication while on the far side of the moon. Eventually it would be desirable to have surface gravimetry traverses over long distances on both the near and far sides.

Seismology provides the most direct information about the tectonic activity and internal structure of a terrestrial planet. Seismic waves generated by natural or artificial sources can be analyzed in terms of travel times of body waves and dispersive properties of surface waves and by free oscillation periods. From seismic velocities and density as a function of radial distance one may infer the presence of major discontinuities, the existence of a lunar crust or core, the physical state of each structural unit, and, to some extent, the composition.

The events recorded to date by lunar seismometers are significantly different from those observed on earth, and the moon appears to be less seismic by at least an order of magnitude. Seismological investigation of the moon, therefore, will require a net of at least three widely spaced seismic stations that must operate for some years in order to provide sufficient spatial and temporal resolution of the signals. Artificial seismic sources, such as Lunar Module and Saturn IVB impacts, will play an important role in these studies. Active seismic experiments planned for later Apollo missions are expected to provide data on thicknesses of the lunar surface debris, basalt flows, and other near-surface layering. This information is important for the determination of cratering history and the extent of differentiation.

Studies of lunar magnetism are aimed at understanding the constraints that paleomagnetism places on the early chronology and origin of the moon, while the electromagnetic response of the moon to induction by

the solar wind may provide a profile of its bulk electrical conductivity. These data may also lead to some knowledge of the contemporaneous thermal, chemical, and physical profiles—including whether there is water or ice at accessible depths beneath the lunar surface.

High-coercivity, paleomagnetic, nonmeteoritic iron has been found at both lunar landing sites, and the Apollo 12 magnetometer shows a relatively strong local magnetic field. Rb–Sr ages, together with lunar paleomagnetism, indicate the presence of a magnetic field at the time of the “setting” of the Rb–Sr “clock” about 3.6 billion years ago. Thus, the explanation of the field source within the general framework of lunar evolution in the epoch about 1 billion years after the formation of the solar system becomes a key issue for further study. The candidate sources for such a magnetizing field are an interplanetary field, a self-excited lunar dynamo, meteor-strike-induced magnetism, or a close approach to earth with immersion in the geomagnetic field. Continuing paleomagnetic study of lunar samples and deployment of additional magnetometers on the moon are necessary to answer such questions.

The considerations outlined above, tempered by an awareness of financial and societal factors, lead us to suggest three options for the continuing exploration of the moon.

The following assumptions are common to all options:

1. All six* remaining Apollo flights will be completed. (Launch intervals may be reduced.)
2. Sufficient funds will be provided by the Apollo program and the NASA Office of Space Science and Applications for continued analysis of Apollo samples and data through 1980.
3. The time schedule, scientific payload, and target sites of automated missions will depend on the results of the Apollo data.

The options beyond Apollo are:

1. *Automated Lunar Program (ALP) with orbiters, landers, rover traversing, and sample return capability.* This is seen as the next truly significant step in lunar exploration beyond Apollo; it would provide data about the moon at local, regional, and global levels. Five orbiters would each carry a 100-kg science payload, with relay tracking satellites for gravity surveys, electromagnetic sounding, infrared spectroscopy, and side-looking radar. Each of five landers would put down a 1500-kg science payload that would include an Apollo Lunar Surface Equipment

* Reduced to four since the study.

Package (ALSEP)-type stationary science station, a 300-kg rover that could carry a science payload and collect samples over a traverse distance of 100 km, and a sample return system that could send 15 kg of lunar material back to earth. This option has great scientific capability. The information it would produce in many fields, added to the information obtained by the Apollo program, should greatly refine our concepts of the moon, the early earth, and the solar system. It would also serve as a conceptual, developmental, and testing exercise for eventual automated missions to Mars and perhaps to other planets and satellites. The estimated cost over a 10-year period would amount to about \$1.4 billion, a small fraction of Apollo costs.

Remote-control mechanisms have important applications in the field of lunar and planetary research and exploration. Remote driving of vehicles, handling and examination of rock specimens, manipulation of laboratory apparatus, and the like, are techniques that have had little development within the space program up to the present, chiefly because they were not needed for lunar exploration because of the manned Apollo program. This situation is now changed because the Apollo program will end before 1975. The planetary program contains the first round of soft-landing vehicles on Mars, and their successors will require greatly augmented remote-control capabilities long before any manned Mars program is undertaken. There is a clear case for doing by remote-control technology those tasks on the moon and Mars that will not, in the foreseeable future, be done by men.

The philosophy of remote control is very different from that of automation; here the device provides no substitute for human judgment and knowledge, and no decisions are automated. Instead, all visual information that would be available to a person in the remote location is sensed and telemetered, and all mechanical actions performed by the operator are caused to activate the appropriate devices of the remote instrument. The telefactor is one general-purpose version of these principles, but many special-purpose versions can also be envisioned. This line of technology will provide manned capabilities in remote places without imposing on man the burden of the risk and the long travel times, and it most probably will be far less expensive.

We therefore strongly urge that a vigorous program be undertaken now to develop these techniques, and that their application to the moon and Mars be reviewed within the next two years.

2. *Automated Lunar Program (ALP) with orbiters and landers only.* This is a scaled-down version of Option 1 and does not include traverse or sample-return capability. It would include five soft landers capable of putting down at any site on the moon with 25 kg of science payload, with

a Radioactive Thermo-isotope Generation (RTG) power source and a lifetime of at least a year. Data to be returned include composition by x-ray and alpha-scattering techniques, other chemistry, seismometry, magnetism, and TV imagery. Two orbiters, each with 60-kg science payload and relay tracking satellites would return gravity data and remotely sense surface features. Magnetic-field observations could also be carried out with this orbiter-tracking satellite configuration. Option 2 would cost about half as much as Option 1 over a similar 10-year period. Its disadvantage is that it makes no traverses and returns no samples, both essential factors to the extension of the geochronological and regional geochemical data net. However, it would return very useful geophysical and local geochemical data; and, funds or other priorities prohibiting Option 1, it could include provisions for leading into an advanced automated program when practicable.

3. *Orbiter with relay satellite for gravity investigations.* Density anomalies on the moon can be and have been investigated by gravity surveys; but data are needed from the far side, and more detail is needed from parts of the near side of the moon. Density variations found can be interpreted in terms of the physical state of the lunar interior, temperature profiles, and internal structure. Observations to be made should also improve the accuracy of the low-order harmonic coefficients of the lunar gravitational field. The orbiter with tracking relay satellite is needed to get data from the far side. The primary merit of this option is that it is the lowest cost option that will return useful data from the moon beyond that obtained by the Apollo program.

A condensed comparison of the above options is given in Table 2.

Recommended Program

We endorse the major recommendations of the 1969 Summer Study on Lunar Exploration (see *Lunar Exploration: Strategy for Research 1969-1975*, pp. 7-9), except for the emphasis in their Recommendation 4 on the longer spacing of missions. We propose a closer spacing of missions rather than cancellation of any part of the remaining Apollo program. The central priorities as we now see them are:

1. Completion of all remaining Apollo missions through Apollo 19* should take highest priority in the space program. The Apollo program has made a good start. The richness of scientific data obtained has pro-

* Since this study Apollo 15 and Apollo 19 have been canceled.

vided important constraints for redefining old questions and for asking significant new ones, but a better sampling program and a broader geophysical net are needed to resolve those questions. As only eight landings will have been made, if all remaining missions are completed successfully, the loss of any missions will seriously degrade the baseline for future lunar and other planetary exploration—especially the later missions with longer stay-times and extravehicular work and with more complete instrument packages. Several factors make it seem improbable to us that Apollo can be interrupted and successfully restarted later. To abbreviate further, or to postpone, a program so fruitful, so well conceived, and with so much promise for resolving fundamental questions would, in our opinion, be irresponsible. We have considered the consequences of this view for Skylab, and we remain deeply committed to Apollo, even if it means delaying Skylab until a later opportunity. Should the Apollo program for any reason be further reduced, then priority 4, below, would move up to the highest level, with the goal of obtaining as much as possible of the needed data in an automated mode.

2. Continuation of sample analysis and data reduction at a viable level for at least five to eight years beyond the last Apollo mission is an essential part of the Apollo program. To sustain the present level of support, or even to move to a viable lower level, requires the provision of funds for that purpose beyond the present level of OSSA funding. We recommend, therefore, that an Apollo budget of at least \$15 million a year be continued as a line item, specifically for data analysis, for at least three or four years beyond the last Apollo flight. This would permit the program to survive at a reduced but sustainable level and to phase gradually to a lower level of continuing support. We assume that the functions of the Lunar Receiving Laboratory and facilities for tracking and telemetry will be separately budgeted.

3. Recommendations previously made for increasing the role of scientists in the Apollo program should play an important role in the selection of future astronaut teams, other considerations being equal. The excellent results obtained to date are a tribute to the judgment and enthusiasm of the Apollo 11 and 12 crews; but a geoscientist astronaut on the lunar surface could make even more discriminating observations and sample selection. There is no substitute for experience and problem-oriented involvement in maximizing chances of obtaining data of the highest resolution. This will be especially important in the later missions.

4. It would be highly desirable to initiate a well-conceived Automated Lunar Program at an appropriate time in the future—and urgent to do so promptly should any of the remaining Apollo missions be lost. This

TABLE 2 Summary of Lunar Program Options

Program	Description	Candidate Experiment	Number of Missions	Launch Dates
Basic program common to all options	1. Completion of all Apollo flights through Apollo 19^a 2. Continuation of the analysis of data and samples through 1980 with partial support from continuing Apollo line item	Apollo as programmed	6 additional Apollo landings ^a	1971-1973
<i>Automated Lunar Program</i>				
Option 1	1. Study surface and interior by an advanced orbiter 2. Land scientific stations comparable to ALSEP	Orbiter: Same as Option 2, plus electromagnetic sounding, infrared, altimetry Landed science: Similar to Apollo ALSEP for study of interior, imagery, bulk composition, and geochemistry	5 orbiters 5 landers	1978-1982

Option 2	3. Traverse science (rover) with 1000-km range	Traverse science: Deployment of instrument arrays, regional geological, geochemical, and geophysical studies		
	4. Sample return to earth	Sample return: Laboratory analysis of samples collected during traversing for age, composition, evolution, etc.		
	1. Study of lunar surface features and internal properties by orbiter	Orbiter: Imagery, gravity, magnetics, composition by remote sensing	2 orbiters	1978-1982
Option 3	2. Deployment of a landed science package	Lander: Imagery, geophysical package for study of interior, composition analysis	5 landers	
	Mapping of the gravity field of the moon including the far side	Gravity field of the moon (harmonic coefficients)	2 orbiters	1976-1977
		Lunar mascons on the far side		

* Since this study Apollo 15 and Apollo 19 have been canceled.

calls for preliminary logistical studies as well as a mission-by-mission integration of results obtained, so that, as Apollo draws to a close, evaluation can be made of appropriate goals, priorities, costs, and time of initiation that will best fit with other objectives of the scientific exploration of space. The components of such a program at two different levels are discussed above. It would be most effective if it were to include rover traversing and sample return capability.

5 Astronomy

Astronomy is the study of the extent and behavior of the universe, of the beginnings, evolution, and ultimate fate of matter. At some very early time, after men realized that there was regularity in the motions of the planets, sun, moon, and stars, the idea of precise measurements of physical phenomena was born. As measurements were continually refined, the subtleties of the observed regularities led to new formulations of physical laws and to entirely new concepts of man's place in the universe. At the same time, astronomy was put to use directly for navigation and time-keeping, while the fruits of astronomy, the new physical laws, led to far-reaching changes in men's lives, just as the exact sciences, starting with Newton's laws, paved the way for the industrial revolution and modern technology.

Recent advances in astronomy have been intimately linked with the most fundamental problems of modern science. Einstein's far-reaching theories about gravitation and the geometrical properties of space came only a few years before Hubble discovered the universal expansion of the universe. Suddenly, where astronomers had only the faintest expectations of searching out the origins of the universe, new experimental evidence and theoretical understanding brought solutions much closer to hand. Again in the past few years, the discovery of the celestial microwave radio background has supported the picture of the universe starting as an immensely dense concentration of matter and all the present galaxies being the remnants of the primordial explosion of this matter. The grand

Members of this Working Group were B. Burke, *Chairman*; J. Blamont, H. Bondi, T. Chubb, G. Clark, R. Danielson, R. Giacconi, L. Goldberg, H. Gursky, N. Mayall, P. Meyers, L. Peterson, E. Spiegel, L. Spitzer, R. Tousey, H. van de Hulst, J. Wilcox, and N. Woolf.

question of the origin of the universe is not settled, of course, but the evidence in favor of an extremely dense initial state (the so-called "big bang theory") is mounting. Already, interesting complications have been raised by space measurements of the radio background at 1-mm wavelength and shorter, wavelengths that are strongly absorbed by the atmosphere of the earth. This typifies the progress of science, as new discoveries give new insights, suggest new tests, and then raise new problems. Each advance enlarges our view, but we then see fresh lands to explore from the new vantage point.

The advent of space vehicles which can carry instruments above the atmosphere opens vast new windows in the electromagnetic spectrum. It extended observations that were previously inaccessible to short wavelengths in the ultraviolet, x-ray, and gamma-ray regions, to the infrared, and to very long radio wavelengths. It provides access to the high-energy particles accelerated by many astronomical objects. These new tools are vital to the search for the ultimate beginnings of things and for efforts to describe the entire universe. The birth and death of stars, the formation of planetary systems, and the synthesis of the elements are among the fundamental problems now being attacked through these new windows.

A telescope in space not only has a larger range of accessible wavelengths for observation but is freed from other limitations. The atmosphere distorts the images of stars and contributes a background glow as well. Among the many new problems that can be approached by a telescope in orbit is the measurement of the distances and distribution of other galaxies with much greater accuracy than is possible from the surface of the earth. In view of the fact that one aspect of Einstein's general theory of relativity is its linkage of the distribution of matter to the geometrical properties of space, optical observations, which measure distance as well as angle, are uniquely capable of investigating this aspect of the fundamental properties of the universe.

Already, new classes of astronomical phenomena of enormously high energy have been revealed by x-ray and gamma-ray astronomy that could only be performed by instruments above the earth's atmosphere. A background glow of x rays has been revealed, an observation that intrigues the theorist because it may hold the clue to the existence of a hot intergalactic gas containing most of the matter of the universe. The properties of the gas may decide whether Einstein's ideas about gravitation are correct or whether that theory, too, must be modified.

Our own sun, our closest star, holds special interest for astronomy. Besides its dominating effects on life on earth, the sun provides some of the sharpest tests for stellar astrophysics because of the wealth of physi-

cal processes that can be observed. Here, there is a real expectation that astronomers are not only seeking the answers to fundamental scientific problems but are developing skills in areas of applied physics as well. The eventual need for new sources of energy to serve man on earth would be amply filled by successful containment of a hot plasma capable of sustaining nuclear fusion. That energy could be generated in this manner was first recognized when the source of solar energy was explained in the 1930's in terms of nuclear energy. The necessary fusion reactions now await the mastering of plasma containment, and astronomers have been vigorous participants in theoretical and laboratory efforts in this field. The difficult task of understanding all the instabilities that plague the laboratory experiments may well be solved when we understand the astrophysical processes.

The dual motivation of basic and applied science—the desire to achieve an ever-expanding knowledge of the intricate but orderly structure of the universe, combined with the expectation that the fundamental addition to the laws of nature that must come from this program will yield real, practical benefits in the long run—has guided this Working Group in the construction of a balanced, broad-front approach to the challenge of space astronomy.

RECOMMENDED PROGRAM IN ASTRONOMY

The Astronomy Working Group has addressed the problem of priorities in space astronomy by a twofold approach. First, we have tried to develop clear, general principles to guide the decision-making process. Second, we are in complete agreement that nothing short of the minimum recommendation of the NASA Astronomy Missions Board (AMB) (*A Long-Range Program in Space Astronomy*) can maintain a truly balanced and viable program of astronomical research in space. The AMB, after several years of careful study, recommended a minimum program at \$250 million per year (Table 3) comparable with the lunar and planetary programs. This level represents careful balance and even development among the various subdisciplines and is agreed upon by the astronomical community. We have formulated a program at lower budget levels at the request of the Executive Committee, but we emphasize that our conclusions are not to be interpreted as an updated or improved version of the AMB report.

Agreement was reached very quickly on guiding principles. These are stated as our major recommendations:

1. A broad-based program in space astronomy is essential in order to advance observations in all regions of the spectrum. Understanding of

10-km radio 5 sec
of arc solar

XOR SP-RA XOR SP-m XOR SP

OXO

Oru

2 p 4

KA
C SO
32

ASTRA 1 sec of
arc solar

XC

ORU

2 p. m.

III-
C O
32

XO

5-10-m LST 0.1
sec of arc solar

×○

RS

p

In a

Par
Evo

0AO-E^b
0AO-E

0AO-D

ATM-A
OAO-D

D-CHO

OXO-A

0AO-F
0RO-A

5"-B

5"-C

OXO-B

0A0-G

2

5"-D

ORO-B
5"-E

2

ASTRA-A

ASTRA-B

LST→

0.1" $\uparrow$

^a In column 1, X, x- and gamma-ray astronomy; O, optical ultraviolet and infrared astronomy; R, radio astronomy; S, solar astronomy; P, planetary astronomy.

^b Partial use.

c Evolves to effect 1" observations in 1980's.

astronomical phenomena requires, as a rule, observations of the phenomena in many different regions of the electromagnetic spectrum because the characteristic energies, time, distance scales, and degree of order vary so widely. We urge that this principle be adhered to, even if it requires that a series of smaller instruments with wideband coverage be chosen over a single very large instrument usable over a restricted wavelength range.

2. If available funds for space astronomy contract, we urge that the support for rockets, balloons, airplanes, and laboratory work be maintained and even expanded.

3. The next major new start should be the High Energy Astronomical Observatory (HEAO) series of satellites.

4. The low-level program that we developed is summarized in Table 4. The total estimated yearly level is \$161 million. The program reflects the conviction that new advances, using large instruments, must go hand in hand with exploratory programs that can be adapted rapidly in the light of new findings. A continuing series of smaller experiments and observing instruments is essential to guide scientific planning, to

TABLE 4 Low-Level Astronomy Program (\$161 million) Developed in this Study

Program	Approximate Cost Allocations, % ^a	Launches ^b									
		72	73	74	75	76	77	78	79	80	81
Runout	1										
SRT, rockets, etc.	22										
Ground based	7										
Small satellite (SAS)	7	X	X	X	X	X	X	X	X	X	X
Solar (OSO, 1")	16		I	J	K			L	M		
High energy (HEAO, proto-OSO)	19			A	B	C		D			
Ultraviolet, optical (1.5-m proto-LST)	21							1.5 m			
Infrared (OIRO)	4									OIRO	
Radio (VVLBI, KWOT)	3				VVLBI						KWOT
TOTAL	100%										

^a Cost allocations are approximate and are based on cost estimates provided by NASA.

^b Launch dates in several instances are approximate and can be adjusted when required by programming considerations.

explore new ideas, and to test new instrumentation, i.e., to ensure the most efficient and productive use of the larger instruments on space missions. The launch dates in several instances could be adjusted when programming consideration require it. The "minimum program" of the Astronomy Missions Board would represent a far more desirable program, and a more restricted budget level has forced difficult priority decisions. Solar astronomy would be supported at only half the AMB recommended level, with the proposed 0.1 sec of arc solar space observatory deferred indefinitely. The uv/optical program is severely restricted, because no intermediate program between the present Orbiting Astronomical Observatory (OAO) series and the proposed Large Space Telescope (LST) program can be supported at the lower level. A serious gap in uv observations in the mid-1970's is a certain consequence. The x-ray program is supported at a lower level, and the proposed x-ray observatory of the AMB program is indefinitely postponed. There is an especially grave curtailment of the small astronomy satellite (sas) program, which progresses at the rate of one or less per year, compared with the three or four per year recommended by the AMB.

Critical Issues

In addition to the four major recommendations listed above, we have identified several critical issues which we list below as specific recommendations. This list is not complete but is intended to highlight important issues that might otherwise be ignored.

Particles and Fields

We endorse those solar-terrestrial physics and planetary missions that are applicable to particle astronomy. We concur in the AMB recommendation that 20–25 percent of these payloads be assigned to particle astronomy.

Rockets and Balloons

We support National Academy of Sciences recommendations made at the 1965 Woods Hole Study (*Space Research: Directions for the Future*) and in 1969 by the Committee on Rocket Research (*Sounding Rockets: Their Role in Space Research*) that the rocket program be doubled. We urge that these recommendations be implemented and that they be extended to the balloon program.

Ground-Based Astronomy

Support of ground-based facilities is strongly justified to complement space missions, and resources should be allocated following the major

recommendations of the AMB and of the SSB Planetary Astronomy Panel (*Planetary Astronomy: An Appraisal of Ground-Based Opportunities*). We recommend that such support be at a level of \$12 million a year for the ten-year period.

Solar Physics

We recommend active support of design studies for a solar observatory with stability of 1 sec of arc or better and payload capacity comparable to ATM-A. The solar observatory should be ready for flight in the late 1970's.

High Energy Astronomy Observatory (HEAO)

We recommend that the first group of approved HEAO missions include both scanning and pointed payloads, the latter with an accuracy of 1 min of arc.

Large Space Telescope (LST)

In order to verify the stability of ultra-low-expansion materials before the proto-LST design must be frozen, we recommend that at least one ultra-low-expansion mirror in the 1.5-m class be constructed, figured to diffraction-limited tolerance, and environmentally tested as soon as possible.

Small Astronomy Satellite (SAS)

We recommend that the SAS program retain its original objective of rapid follow-up of new discoveries, and that the emphasis be on the largest number of flight opportunities within budgetary constraints.

Future Manned Experiments

Space stations offer great opportunity for solar research because of the high data rates inherent to solar observations.

DISCUSSION OF RECOMMENDED PROGRAM

Rockets, Balloons, Aircraft, Data Reduction, and Supporting Research and Technology (SR&T)

The combination of SR&T, data reduction, and the rocket, balloon, and aircraft astronomy programs—the nonorbital space astronomy program, not including ground-based astronomy—amounts to \$35 million per year in the overall astronomy program of this exercise. The nonorbital pro-

gram is the only part of the space astronomy program that does not vary with assumed budgetary level. It contains roughly a doubling of the rocket-balloon program funding. The rocket and balloon program is universally supported by astronomy groups for at least the following reasons: its record of achievement in producing results of astronomical significance; its flexibility and ability to capitalize on the ingenuity of the investigator in obtaining new results; the relative short time scale of each experiment, which is helpful in permitting quick rewards from technological advances, correcting mistakes, and educating students; and its value in the calibration of orbiting instruments.

The value of the nonorbital program is enhanced by substantial improvements in supporting technology: attitude control systems that make 1 sec of arc studies possible; recovery systems that provide a high probability of rocket payload recovery; larger balloons that permit studies at 1 g cm⁻² overhead air mass; and aircraft hardware that makes possible infrared studies above the tropopause with a 36-in.-diameter telescope. These developments mean that the nonorbital space program is ripe for production of new astronomical results. Indeed, in those areas in which studies are not severely quantum limited, e.g., solar and infrared astronomy, the rocket is considered by some as a challenger to the orbiter for retrieving supplementary data. In most areas, balloon or rocket programs can be expected to provide significant pioneering data over the next decade.

We make no recommendation on the division of funds among rockets, balloons, and aircraft. We note that, as pointed out by the Space Science Board's Committee on Rocket Research (*Sounding Rockets: Their Role in Space Research*), the value of the rocket program could be increased if the Black Brant V or equivalent could be fired at White Sands (because of its larger payload-altitude capability at equal cost relative to the Aerobee). For solar work, continued ability to launch Aerobee-size vehicles at will on observation of a flare is considered important.

High-Energy Astronomy

High-energy photon astronomy has developed during the past decade into one of the most fruitful areas of modern astronomy. Experiments conducted above the obscuring atmosphere have extended observations beyond the uv by more than six decades of the spectrum and have resulted in the discovery of surprising and unexpected sources of celestial x and gamma rays. These discoveries have already had a major impact on our ideas regarding the origin and early history of the universe, stellar and galactic evolution, the properties of the interstellar medium, and the

origins of cosmic rays. The promise of continued important advances and the challenge of the observational problems have attracted many capable experimentalists to the field and have inspired the development of a battery of new techniques for the detection and analysis of radiation in space. Thus the necessary foundation exists for a program that will exploit these recent scientific breakthroughs during the coming decade through the use of much larger space instruments. Meanwhile, a sustained program of smaller exploratory investigations will continue to stimulate new technical developments and assure a continuing yield of new discovery.

There is wide agreement in the astronomical community that major new advances will require the use of very large spacecraft capable of carrying many thousands of pounds of scientific payload into near-earth orbit. Although it is deemed essential that exploratory work in this new field be continued with balloons, rockets, and small satellites of the SAS class, there are specific observational objectives that can be accomplished only with a large new spacecraft. To extend substantially the sensitivity of the all-sky x-ray surveys that will emerge from SAS-A, OSO-H, and SAS-C, and thereby to study the x-ray emission of fainter galactic objects and more distant exterior galaxies, requires a detector with a sensitive area of ten or more square meters. A massive scintillation detector or cryogenically cooled solid-state detector with active anticoincidence shielding is necessary to study the nuclear gamma-ray lines expected from supernova remnants. A gamma-ray spark chamber with a sensitive area greater than 1 m^2 and a weight of several thousand pounds will be required to analyze the distribution of the high-energy gamma-ray emission of the galaxy and to measure the gamma-ray luminosity of extragalactic sources with a sensitivity substantially better than that of the SAS-B experiment. A large focusing x-ray telescope and auxiliary instruments in a payload weighing several tons will be required to exploit the existing and flight-proven technology of high-resolution (several seconds of arc) x-ray imaging by grazing-incidence optics and to perform high-resolution x-ray Bragg spectroscopy and polarimetry on individual sources.

All the above requirements go far beyond the capabilities of small Explorer-class satellites. They can, however, be met by the projected High Energy Astronomy Observatory (HEAO) in two closely related versions. The first is a rotating "unpointed" version which is well suited to high-sensitivity x- and gamma-ray survey experiments as well as to the heavy experiments required for particle astronomy. The second is the "pointed" version which needs only 1 min of arc pointing control to accommodate the high-resolution x-ray telescope. In this second version,

the image motion caused by the residual 1 min of arc orientation drift of the spacecraft will be precisely compensated by electronic image processing to achieve the full second of arc resolution of the telescope.

Particle Astronomy

Particle astronomy, or cosmic-ray research, plays an important role in modern astrophysics and astronomy. The scope and program may be divided into three major areas: galactic, interplanetary, and solar physics.

Insight into questions of galactic physics is gained from the flux, energy spectra, and composition of cosmic radiation. Cosmic-ray particles are the only form of matter known to reach the earth from outside the solar system, and their composition carries the signature of their sources. It has become increasingly clear that this composition points to a thermonuclear origin. Measurements of the high-energy electron and positron spectrum (to 1000 GeV and beyond) will shed light on such astrophysical questions as the lifetime of the cosmic-ray particles in the galaxy and the density and the source distribution of photons. These problems are intimately connected with the problems of radio, x-ray, and gamma-ray astronomy. Experimental techniques are at the threshold of providing adequate discrimination to permit resolution of isotope composition in the cosmic radiation for nuclei heavier than hydrogen and helium. This possibility not only permits a further identification of cosmic-ray sources but also provides clocks in the form of long-lived radioactive isotopes capable of measuring the average cosmic-ray life. The cosmic radiation is one of four important factors in the determination of the state of the interstellar medium (the other three are magnetic fields, neutral gas, and starlight). Cosmic rays play a role in star formation and in the heating of H I regions. While gamma-ray evidence has shown that the flux of antiparticles cannot be large, it is of fundamental interest to learn how small this flux is. The HEAO program is necessary to attain many of the above-mentioned goals. Detectors of large mass are needed to measure particle energies up to 10^{13} eV. At the same time, large detection areas are necessary because this particle flux rapidly decreases with increasing energy. Extremely large counter areas and long exposure times are needed to study nuclei with charges heavier than iron, because these particles have a flux of the order of only 10^{-4} of the Fe-group nuclei. The goal of separating positively and negatively charged particles in a magnetic spectrometer using a superconducting magnet can only be attained on a spacecraft of the HEAO type. The technology for most of these experiments is developed and only awaits a spacecraft to be cast into flight hardware.

Studies of cosmic rays in interplanetary space are important for two reasons: solar modulation prevents full access of the low-energy interstellar cosmic-ray flux to the inner solar system, and measurement of the spatial dependence of the cosmic-ray intensity in interplanetary space is a powerful tool to probe the configuration of interplanetary magnetic fields. For example, solar and galactic particles can be uniquely discriminated by investigating the spatial dependence of isotopes such as ^3He . Such isotopes are known to be very rare on the sun, and their detection assures a sample of galactic particles free of possible solar contamination.

Energetic particles from the sun are an outstanding manifestation of solar activity. Coordinated studies of solar particles (nuclei and electrons), x rays, and uv, radio, and optical emissions contribute to the understanding of the solar particle acceleration process. While solar flares are the most prominent source of particle emission, active solar regions are now known to emit energetic particles almost continuously. The mechanisms leading to this emission are little understood. High fluxes of solar particles are a danger to men in space and supersonic flight. Fulfillment of these tasks in solar and interplanetary physics will rely heavily on eccentric satellites (IMP; see Chapter 7) as well as on interplanetary missions (Pioneer, Grand Tour; see Chapter 3).

Solar Astronomy

The proposed solar program includes study of the solar chromosphere and corona and of the processes of solar activity. The solar corona extends into interplanetary space as the solar wind and immerses the earth in its stream. The corona and chromosphere are caused by physical processes, not presently understood, in which the mechanical energy of solar convection is converted to thermal energy in the coronal gas at million-degree temperatures. Also to be understood are many solar-activity phenomena, in particular the flare process in which a sudden release of energy heats solar gas to some 20 million K, massive propulsion of solar material occurs, and particles can be accelerated to relativistic energies. The sun provides many natural laboratories in which plasma processes can be observed, and the use of these laboratories to deduce laws of energy conversion under conditions in which magneto-hydrodynamic processes are dominant is one of the major aims of the next decade of solar research.

The past decade of space research has given solar astronomy considerable momentum and a sound basis of technology and physical knowledge from which the continuing program has been derived. The

solar program of the next decade is built on the fruits of those past developments. Its goal is to attack the major problems in solar physics such as the coronal heating and solar activity. One component of the program is the spectrophotometric analysis of small, relatively homogeneous regions on the sun throughout x-ray and uv wavelengths. This requires a platform similar to the Apollo Telescope Mount (ATM), but with 1 sec of arc pointing. The other component is the continuing Orbiting Solar Observatory program, which provides a basis for the time-dependent studies and the solar observations that do not require the full angular resolution of the 1 sec of arc observatory. Among these are coronal and line-profile studies, active-center histories, coronal magnetic structures, and flare plasma studies. Both these approaches, together with continuing rocket and balloon work (e.g., for hard x-ray oscillations, coronal streamers, searching for nuclear gamma-ray emissions), are needed if the present promise of the program is to be realized.

The program also has provision for solar monitoring both because of its intrinsic interest for solar physics and because of the direct effect of solar radiation and particles on the earth. NASA has contributed to solar activity monitoring in the past by small experiments on OSO and OGO and by provision of launch and data support to the Naval Research Laboratory SOLRAD program. Ultraviolet monitoring more specifically oriented to ionospheric *F*-region studies will be carried out by Atmospheric Explorer (see Chapter 7) and SOLRAD 10. Line-profile studies, which are necessary to interpret the geocorona and the dayglow, are contained in OSO. Monitoring of the solar wind and energetic particle flux is provided by IMP and is proposed for the Solar-Terrestrial Probe (see Chapter 7). Additional total-disk monitoring that will furnish more detailed and accurate data throughout the extreme ultraviolet wavelengths, and total-disk line-profile data on key lines including helium, are needed in support of the solar-terrestrial physics program.

We emphasize the close relation between solar physics and the observations of the solar corona near the orbit of earth. Solar-wind plasma, interplanetary magnetic fields, and energetic solar particles are observed by the IMP's, by the proposed Solar-Terrestrial Probe, and on planetary missions. The solar-physics knowledge obtained in this way nicely complements the observations obtained with OSO's and related satellites. We also recognize the essential contribution of coordinated ground observations to the solar program.

The above program does not include the very interesting goal of studying the solar gravity field (see Chapter 6). Future planning should recognize that this area is currently in controversy concerning the size of the quadrupole component. Eccentric solar probes should contribute

significantly to an understanding of the solar interior by measuring the solar gravity field.

Optical Astronomy

The central aim of the optical astronomy program is a 3-m diffraction-limited telescope known as the Large Space Telescope (LST). The wide range of problems that can be attacked by this powerful, permanent instrument has been carefully detailed in *Scientific Uses of the Large Space Telescope* and in *A Long-Range Program in Space Astronomy*. In brief, the LST would make a dominant contribution to our knowledge of cosmology by extending well-known distance indicators, thereby allowing a much better discrimination between theoretical cosmological models. Because of its combined high spatial resolution and large light-gathering power, the LST would provide decisive information in many fields of astronomy including the measurement of the density, composition, and physical state of the galactic halo; the study of the very energetic processes that occur in galactic nuclei; the study of the early stages of stellar and solar system formation; and observation of such highly evolved objects as supernova remnants and hot white dwarfs. The LST can also obtain synoptic observations of fine planetary detail over long time periods. The wide spectral range and variety of auxiliary instrumentation make the LST flexible for investigating problems that have not yet been formulated. The offset guiding capability is particularly important in giving this flexibility.

As an intermediate scientific and technological step toward the LST, the optical astronomy program has a mid-1970's goal of a diffraction-limited telescope in the 1.5-m class. The proposed uses of the 3-m telescope are all valid for the 1.5-m instrument, though on a more limited scale. For example, the structure of galactic nuclei having diameters of 0.5 sec of arc and brighter than eighteenth magnitude would be resolved in 1-h exposures with the 1.5-m instrument. Studies of the nuclei of galaxies are at the frontiers of science, and the prospect of opening new horizons seems high in this and other areas. In particular, the possible role of stellar collisions in the very compact nuclei may be assessed.

The feasibility of the LST has been established in a variety of studies. The recent successful flight of Stratoscope II has directly demonstrated the practicality of a 1-m diffraction-limited telescope. That a large observatory can be operated by remote control in near-earth orbit is clear from the experience with OAO-2. The technology to build a 1.5-m telescope is now clearly available.

A technical problem which has been of concern is the long-term

dimensional stability of the primary mirror. There seems little doubt that a solid 1.5-m diffraction-limited mirror constructed of ordinary fused silica will retain its figure for many years in the presence of the expected vibration and thermal cycling. The use of ultra-low-expansion materials (e.g., CerVit or ULE silica) as the primary material would greatly relax the thermal design tolerances, but the dimensional stability of these materials is much less certain. In order to verify the stability of ULE materials before the proto-LST design must be frozen, we recommend that at least one ultra-low-expansion mirror in the 1.5-m class be constructed and figured to diffraction-limited tolerances as soon as possible.

Infrared Astronomy

Current studies at infrared wavelengths are affecting all areas of astronomy. In the early 1960's, astrophysicists were astonished by the tremendous outpouring of energy from radio galaxies since their fantastic power seemed to strain the estimates of nuclear-energy resources. Today we have observations of infrared galaxies, as well as x-ray galaxies, which exceed the power of radio galaxies. Seyfert galaxies have infrared cores a thousand times as bright as all the stars in the Milky Way. An airborne infrared telescope has discovered that the Orion nebula is 100,000 times brighter than the visible sun, even though it is cooler than liquid air. At the nucleus of the galaxy is an infrared source 10 billion times as bright as the sun.

Microwave radio astronomy has given strong support for the "big bang" model of the universe, but to strengthen the case it is essential to extend the measurements into the infrared, where rocket and balloon astronomy have already made primitive surveys. Dying stars are shrouded in dust and molecules which are detected in interstellar space at both radio and ir wavelengths. The new view of 10^{12} earth masses of silicate dust and vast complexes of water and organic molecules in interstellar space casts doubt on concepts of the earth as the unique abode of life.

Very sensitive ir detectors are now operational, and rocket and aircraft sky surveys should be expanded utilizing this equipment, and surveys at the longer wavelengths should be initiated. Following a rocket survey, an infrared SAS would enable us to see ten times deeper into the universe, perhaps permitting the strange bright ir sources to be fitted into a quantitative astronomical framework. Also, very sensitive detectors can be used with a 36-in. airplane telescope and ground telescopes for high-spectral-resolution study of the brightest objects.

However, to obtain an angular resolution of 1 min of arc at $200\text{ }\mu\text{m}$, a 70-cm telescope is required, and limitations on the cryogenics payload

and pointing accuracy probably rule out the SAS class of spacecraft and will no doubt require a special Infrared Observatory.

Radio Astronomy

The radio astronomy program in space, while relatively small, has two important aspects—the observation of the low-frequency cosmic radio spectrum and the achievement of extraordinarily high angular resolution of radio sources through the universe. In minimum form, the low-frequency program would follow the present small missions, Radio Astronomy Explorer (RAE) A and B, with a direct step to a kilometer-wave orbiting telescope (KWOT). The project is technically feasible and scientifically important. The radio power of the galaxy and of many radio sources is still increasing as one goes to lower frequencies, and a large part of the relativistic electron energy density is revealed by measurements of its synchrotron radiation at these frequencies. Self-absorption can be measured, as well, and it will yield unique data on internal conditions in radio sources and interstellar space. An interesting example of the unity of astronomy arises from these measurements: synchrotron radiation by $\sim 10^8$ eV electrons causes the low-frequency galactic radio background and must also be considered in interpreting high-energy gamma-ray measurements at the other end of the electromagnetic spectrum. It should be noted that RAE-B observations, from lunar orbit, will be necessary to establish scintillation limits imposed by the solar wind and to guide planning of KWOT which is planned, therefore, late in the decade.

The second class of radio measurements proposed exploits the techniques of radio interferometry which now are limited by the size of the earth. Very-long-baseline interferometry techniques have shown that quasars contain components that are not resolved at the longest baselines achievable on earth. We propose, therefore, a small radio telescope on a space platform, preferably in an eccentric orbit that extends to great distances from the earth and which can be processed to construct, in effect, an aperture synthesis system that would map quasars to a resolution of the order of 10^{-5} sec of arc (i.e., a resolution of one light year at the limit of the visible universe, for Euclidean geometry). The experience from earth-based interferometers demonstrates that the earth's atmospheric effects are not important, and no technological barriers to a very very long baseline interferometer (VVLBI) appear to exist.

Small Satellites

Small satellites in the 10- to 100-kg class provide flexibility, rapid follow-up of important discoveries, and intermediate steps between

sounding rockets, balloons, and large observatory experiments. They permit exploratory research which may or may not be followed by a major instrument. These opportunities may be part of larger satellites, such as OSO or IMP, or they may be single dedicated missions such as SAS.

The Small Astronomy Satellite (SAS) program originated with the necessity to provide the new fields of x- and gamma-ray astronomy with satellite observational capability. The characteristics of this spacecraft, namely, 100-kg payload, inertial stabilization, arbitrary pointing capability, and near-earth operation, were also recognized as suitable for a great variety of astronomical investigations. We have identified, in several fields, experiments requiring a dedicated spacecraft and quick turnaround, as examples of candidates that are not well suited to HEAO. These include: (1) high-energy missions to study details of individual objects with high angular resolution and to extend the range of energies observable; (2) an optical mission, for broadband uv photometry and polarimetry of discrete objects; and (3) a cooled ir telescope of intermediate lifetime (2-4 weeks) for an ir sky survey. One should note that the character of SAS missions should be exploration, and one should not commit the next ten years of such missions this far in advance. The ease with which one can identify worthy missions at this time reinforces our belief that the proposed small astronomy satellite program will continue to be fruitful.

Small satellite opportunities in the particles-and-fields portion of the astronomy program also appear as part of the planetary and the solar-terrestrial physics programs. They include planetary and interplanetary Explorers, solar and interplanetary probes, flyby and orbiter missions, and IMP. The AMB report identified about 35 such spacecraft missions of which perhaps half of that number are still being actively considered.

Since the inception of the OSO program, the OSO-wheel has been carrying significant nonsolar experiments in the 10-kg class. To cite one outstanding example, the OSO-3 gamma-ray experiment provided the first observational evidence for the existence of cosmic gamma rays. This tradition is expected to continue, particularly with the expanded capability of the wheel. OSO-I, for example, will carry a number of advanced experiments for studying cosmic x-ray phenomena including an experiment to survey very soft x rays below 1 keV, an x-ray polarimeter, and a Bragg crystal spectrometer to search for line emission.

The minimum program recommended by the AMB identified some 40 small satellite opportunities which were included in every sub-discipline. The low-level program described by this Working Group provides approximately 14 such opportunities in the ten-year period, including five in the OSO wheel. Thus this portion of the astronomy program has been reduced by a much larger factor than the total.

Theoretical Astronomy

The budgetary limitations on space exploration make it all the more important to maintain a vigorous theoretical program to obtain maximum return from the data and to help optimize the use of available instruments. The most exciting advances in science have occurred when theory and observation were closely linked, with each mutually guiding the other. Support for complementary theoretical work is needed both in NASA and in the home institutions of the principal investigators. Cooperation between the NASA Theoretical Division and university scientists should be nourished, and astronomical work at the Goddard Institute for Space Studies (GISS) should be encouraged and further enriched. The IBM 360-95 computers at Goddard Space Flight Center and GISS are extremely valuable for theoretical work in support of the space experiments and should be used to full capacity by making them available whenever possible to the astronomical community. These machines represent one of the most important facilities in the world for theoretical astrophysics, and their use by qualified workers would immensely bolster theoretical work in stellar evolution, galactic structure theory, atmospheric physics, planetary physics, and other space-related studies.

Ground-Based Facilities

Space and ground-based astronomy are so complementary that neither can advance without interacting strongly with the other. Astrophysical problems do not confine themselves to narrow bands of the electromagnetic spectrum, and their eventual elucidation depends on data obtained from a wide range of techniques. The close interdependence of space and ground-based observations has long been recognized. New and expanded ground-based facilities, in accordance with the major recommendations of the AMB and the NAS, are essential not only to the space astronomy program but to the progress of astronomy as a whole.

6 Gravitational Physics

There is a class of experiments that are distinguished by their fundamental character. Instead of studying a *particular* object or feature such as a crater on Mars, a *universal* entity such as the charge of an electron is determined or verified. This class has the special appeal that what is determined is applicable everywhere. At the same time, such experiments are limited in scope: once the charge of an electron has been established to a certain accuracy, it is pointless to repeat the measurement unless scientific or technical advancements permit a considerable improvement in accuracy.

This chapter is concerned with fundamental experiments on the laws of gravitation that can best be carried out in space. Experimental knowledge of gravitation that goes beyond Newton's theory is extremely limited, and any addition to this knowledge would be of great value. If new knowledge were to contradict the most widely accepted theory—the Einstein theory of general relativity—it would have a most dramatic impact on our understanding of the physical world. At the present time, physics is ripe for such experiments, because features in the universe of non-Newtonian character have recently been discovered or proposed: pulsars, black holes, and gravitational waves.

Pulsars are believed to be rapidly rotating neutron stars. From a gravitational point of view, they differ from most astronomical objects because they have an extremely strong gravitational field which is produced by exceedingly dense matter in very rapid motion. Black holes are the conjectured state of stars that have collapsed and do not emit or reflect light and are observable only through their gravitational effects. Gravitational

Members of this Working Group were J. Blamont and H. Bondi; L. Schiff, consultant.

waves already may have been detected on the earth; because they can travel enormous distances through the densest matter, they may prove to be unique indicators of the nature of otherwise hidden features of the universe. In each of these cases Newton's theory is inadequate because it explains only the gravitational interaction of static masses and neglects the effects of mass motions. These effects can become of overriding significance in the case of massive objects in rapid motion and require a dynamical theory for their explanation.

For more than half a century, Einstein's general theory of relativity has been almost universally accepted as the successor to Newton's theory of gravitation. General relativity (GR) is a dynamical theory which accounts for the production and detection of gravitational waves and which makes definite predictions concerning the gravitational effects produced by arbitrarily large masses in arbitrary motion. Published work to date indicates that the GR corrections to Newton's theory have been verified only to approximately 10 percent accuracy, which is insufficient to distinguish between GR and its principal current rival, the Brans-Dicke or scalar-tensor theory. More accurate experiments are needed because the predictions of these two theories (and probably also of others that might be proposed in the future) diverge more widely when applied to the three cases of pulsars, black holes, and gravitational waves mentioned above.

Two relativity experiments are now under consideration by NASA: earth-orbiting gyroscopes and sun-orbiting spacecraft.* The gyroscope experiment consists of four precisely spherical gyroscopes placed in a specially designed satellite orbiting the earth in an approximately circular polar orbit at an altitude of approximately 800 km. Two of the gyroscopes have their spin axes parallel to the earth's rotation axis, and the other two have their axes perpendicular to the plane of the orbit. These gyroscopes are so constructed that their drift rates, caused by extraneous torques, are of the order of 0.001 sec of arc per year. Such extremely low drift rates can only be achieved when the force supporting the gyroscope is negligible. This is the main reason why the experiment must be performed in space.

According to Newton's theory, the spin axes of all four gyroscopes should maintain fixed directions with respect to distant stars that have negligible motions with respect to very distinct background stars. General relativity and rival theories of gravitation make definite predictions of

* Laser ranging from earth on corner reflectors placed on the moon is primarily intended to search for a secular change in the gravitational constant and is not expected to make major contributions to GR.

nonzero spin precession rates; these rates are different for the two pairs of gyroscopes, and the various theories predict different values. In particular, GR predicts ~ 7 sec of arc per year precession for the first pair (with spin axes parallel to that of the earth) and ~ 0.05 sec of arc per year for the second pair (with spin axes perpendicular to the plane of the orbit). The first pair will suffice to distinguish, to first order, between GR and, say, the Brans-Dicke theory and hence to settle indirectly the question concerning the mass quadrupole moment of the sun. The second pair will have the capability, unique among all relativity experiments performed or proposed to date, of distinguishing between the gravitational fields of the stationary earth and the rotating earth. This is of particular importance because, as noted above, mass motion effects are expected to be especially significant in understanding pulsars, black holes, and gravitational waves.

The relativity gyroscope experiment requires development of technology well beyond the present state of the art. The drift rate in orbit will be about seven orders of magnitude smaller than that of the best earth-bound gyroscope. Because the directional readout makes essential use of the properties of superconductors, a cryogenic environment (at liquid helium temperature) must be maintained during the flight, which will last from several months to a year. The comparison direction, specified by a bright star of negligible or known proper motion, must be determined to ~ 0.001 sec of arc. Because the telescope aperture is only ~ 10 cm, this determination of direction is far beyond the diffraction limit. It is attainable because the location of the center of the star image is limited only by the accuracy with which image dividers can be made and by photon-counting statistics. Finally, it is interesting to note that the whole system is self-calibrating in the sense that the stellar aberration arising from satellite motion around the earth and earth motion around the sun can be accurately calculated.

The sun-orbiting spacecraft is intended to measure the gravitational field close to the sun, where the field is large, through its effect on the motion of the spacecraft and on the propagation of electromagnetic signals. These effects are quite different from those described above. An experiment of this type is now under study by the European Space Research Organization (ESRO), for possible proposal to NASA as a cooperative project; NASA might then assume responsibility for launching and tracking.

Two technologically novel features will probably be incorporated in this experiment. First, the spacecraft will follow a true free-fall trajectory, independent of external influences such as radiation pressure and solar wind. This will be accomplished by slaving the spacecraft to a small proof

mass that is completely enclosed and hence protected from the environment and by compensating external forces with rocket thrusters. (This feature may also be incorporated in the earth-orbiting gyroscope satellite.) Second, the spacecraft will contain an atomic clock, with provision for laser and multiple radio-frequency ranging from the earth. With these features, not only will the electromagnetic signal travel time be measurable with great precision, but also the orbit will provide, with greatly increased accuracy, the kind of second-order gravitational information that thus far has been obtained only from astronomical observations of the orbit of Mercury.

These two sets of measurements, spacecraft orbit and electromagnetic travel time, will between them lead to two important conclusions. First, they will make possible the distinction between GR and other theories through terms of second order. Second, as in the gyroscope experiment, but perhaps more directly, they will provide information on the mass quadrupole moment of the sun. This latter determination will have great astrophysical significance because the mass quadrupole moment is related to the internal angular momentum of the sun and hence to the distribution of the original angular momentum of the proto-sun (i.e., between the condensed sun itself and its planetary system). This in turn can provide a valuable insight into the likelihood of the existence of planetary systems around stars other than the sun.

The rotation of the sun produces mass motion effects on its gravitational field that are characteristically non-Newtonian. These show themselves both in spacecraft orbits and in electromagnetic travel time. A rough estimate shows the change in the travel time of limb-grazing rays to be of the order of 10^{-9} sec, which is about one part in 10^5 of the main relativity retardation. This ratio decreases inversely with the minimum distance of the ray from the center of the sun. Various schemes, including some terrestrial experiments, have been proposed to measure this effect. One of these schemes, not so far studied in any detail, would use two symmetrically placed heliocentric spacecraft to measure any difference in travel time of man-made electromagnetic beams from the earth that would graze the limb and encircle the sun in opposite directions—with and opposed to its sense of rotation. The effect of solar rotation on the orbit of the spacecraft is relatively larger, but it is still only approximately one part in 10^3 of the main relativity effect if the semimajor axis is equal to that of the orbit of Mercury; this ratio decreases inversely as the square root of the semimajor axis for larger orbits. These estimates are the basis for the earlier remark that the earth-orbiting gyroscopes are uniquely capable of detecting mass motion effects.

The gyroscope experiment has been actively under way at Stanford

University since 1963, and detailed contacts have been established with the Marshall Space Flight Center at Huntsville, Alabama. It is hoped that the system will be ready for launch in 1976, possibly with a second flight about two years later. Launching of the complete experiment should be preceded by a flight of some components, which might be piggybacked, about 1974. ESRO proposes that the sun-orbiting spacecraft be launched by NASA about 1976–1977.

RECOMMENDATIONS

1. The earth-orbiting gyroscope experiment should continue to be funded at a level adequate to have it ready to launch in 1976, possibly with a second flight about two years later. It should be subjected to detailed review with regard to both feasibility* and cost at an appropriate time prior to commitment to flight in 1974. The project should be given approved program status at that time if the review is favorable.

2. The sun-orbiting spacecraft experiment should be subjected to detailed review with regard to both feasibility and cost, as soon as practicable after receipt of a proposal from the European Space Research Organization. It should be given approved program status at that time if the review is favorable.

3. The anticipated costs of both the earth-orbiting gyroscope experiment and the sun-orbiting spacecraft experiment are thought to be low enough, and their scientific and technological importance great enough, that both should be performed in this decade.

* Since this Study, an additional effect, which may have a bearing on this experiment, has been reported by B. M. Barker and R. F. O'Connell ["Effect of the Earth's Revolution Around the Sun on the Proposed Gyroscope Test of the Lense-Thirring Effect," *Phys. Rev. Lett.* 25, 1511 (1970)].

7 Solar-Terrestrial Physics

Solar-terrestrial physics is the study of the particle, magnetic field, and radiation environments of the sun and the earth, their interactions, and the dynamic processes involved.

The accomplishments of the past decade in the study of the solar-terrestrial complex have led to a new and revolutionary picture of the earth in space. Hot ionized gas continuously streams from the sun, carrying a solar magnetic field to the earth's orbit and far beyond. A belt of high-energy particles trapped in the geomagnetic field surrounds the earth. An extension of the earth's magnetic field and gas hull stretches like a comet's tail millions of miles in the direction away from the sun. We find the upper atmosphere to be strongly influenced by solar activity and governed by radically different processes in polar regions as compared with those at the equator and midlatitudes.

The highly successful exploratory stage of this field has provided us with a qualitative, morphological description of the environment, and we have identified many of the major physical processes involved. Very recently, and mainly as a result of cooperative analyses of measurements made simultaneously with two or more spacecraft at spatially different positions, a more quantitative picture is emerging, in particular of the complex interactions among the different regions. For instance, the ionosphere, which we originally considered a distinct entity, interacts with the lower atmosphere through gravity waves and with the magnetosphere through electric fields, particle precipitation, and particle ejection. The polar aurora is only one of many manifestations of a fundamental

Members of this working group were H. Booker, *Chairman*; H. Alfvén, J. Blamont, T. Chubb, T. Donahue, V. Eshleman, W. Hanson, H. Massey, M. McElroy, P. Meyer, J. Roederer, F. Scarf, and J. Wilcox.

acceleration process that involves a huge portion of the earth's environment. Properties of the solar wind reflect both small- and large-scale structures of the sun, thus enabling us to study *in situ* the atmosphere of a star. The problem that we are attacking is the fundamental behavior of plasmas on a macroscopic scale; this behavior is relevant not only to the solar-terrestrial complex but to astrophysics as well.

We are close to a major quantitative understanding of the fundamental processes involved in the solar-terrestrial system. The program for the next decade should be aimed toward achieving this understanding. That, in turn, will have a considerable impact on other disciplines such as astronomy and planetology and on applications such as meteorology and re-entry problems in connection with missile detection and defense.

Table 5 is a summary of the recommendations and guidelines for solar-terrestrial physics.

THE EARTH'S UPPER ATMOSPHERE

Our understanding of the behavior of the lower atmosphere, which contains most of the elements essential to life, cannot be complete without a corresponding understanding of the upper atmosphere and the nature of the coupling between them. Although the upper atmosphere contains only a tiny fraction of the mass of the total atmosphere, it is responsible for the absorption of a large amount of solar radiation, and many of the photochemical reactions that take place in it also operate in a greatly diluted form in the lower atmosphere, playing an important role in the chemistry of pollution. The ozone formed photochemically in the upper atmosphere would form a layer only a few millimeters in depth at sea-level pressures, but without its protective presence, life as we know it could not exist in the direct uv radiation of the sun.

The advent of satellites in the late 1950's brought about an enormous increase in our understanding of the earth's upper atmosphere. Prior to that time, exploration of the upper atmosphere had been largely the domain of radio physics, using the ionosonde as a principal tool, and even the sounding rockets that sporadically probed the region tended to be instrumented to investigate ionospheric properties. To the nonspecialist, the atmosphere appeared to consist of a lower region containing weather systems and an upper region that was electrically conducting and had little or no connection with what lay below.

This picture has changed completely in the past decade. The atmosphere can now be regarded as a whole—a true planetary atmosphere—with intimate coupling between adjoining regions. Solar energy is de-

TABLE 5 Recommendations and Guidelines for Solar-Terrestrial Physics

Program	Mission	Priority
<i>Close-out of Existing Programs</i>		
OGO	Magnetospheric physics	} Highest priority at all budget levels
S ³ -A	Magnetospheric physics, with emphasis on storm phenomena	
ISIS	Studies of the topside ionosphere	
<i>Continuing Support</i>		
Data analysis	To exploit as fully as possible the existing store of data accumulated from past missions	Highest priority. Strongly recommended for substantial increase in funding, especially if new programs are severely reduced
SR&T	Supporting research and technology necessary to maintain a viable space program	Highest priority. If new programs are reduced, a substantial increase is recommended to provide piggyback instrumentation for missions in other fields and to assist theoretical research
Sounding rockets	To carry out specific experiments that do not require wide horizontal coverage, to explore regions inaccessible to satellites, and to develop satellite instrumentation	Highest priority. A substantial increase in funding for rocket exploration of the mesosphere and lower thermosphere is recommended
International	To cooperate in space missions of other countries or international groups (e.g., ESRO)	Highest priority; excellent value for money, especially if U.S. programs are reduced
<i>Approved New Programs</i>		
Atmospheric Explorer C, D, and E	To use satellites with a powered capability to explore the upper atmosphere down to altitudes of ~130 km	Highest priority. A team of scientists exists to undertake operation and analysis. Initiation of the program should not be allowed to slip further but could be stretched under severe fiscal limitations
IMP, I, H, J	Magnetospheric physics and cosmic rays	Highest priority. Plans are well advanced, but stretching of IMP-J is recommended if budget is severely limited

TABLE 5 Recommendations and Guidelines for Solar-Terrestrial Physics—*Continued*

Program	Mission	Priority
<i>Proposed New Programs</i>		
Solar-Terrestrial Probe A, B, C	To provide continuous data on interplanetary conditions at a distance of about 10^7 km from earth	A has first priority, B and C second priority. Launches at 3-year centers are recommended. Coordinate A with international magnetospheric program
IMP, KK'	To develop the mother-daughter concept, aimed at separating spatial and temporal effects in magnetospheric phenomena	First priority. Delay only under severe budget restrictions. Coordinate with international magnetospheric program
IMP Cluster L, M, N ^a	To extend the mother-daughter concept to a cluster of four satellites	L has first priority, M second, and N third
Electrodynamic Explorer A, B	To use the AE vehicle to investigate the electrodynamics of the ionosphere-magnetosphere system	Second priority
Atmospheric Explorer F, G, H, I	Follow-ons to AE-C, -D, -E, to exploit their results and to study solar-cycle changes	Second priority to F, G; third priority to H, I
Neutral point Explorer	To study the solar wind-magnetosphere interface at high latitudes	Second priority
Plasmapause Explorer	To explore the physics of the plasmapause	Second priority
Fields and particles studies in unexplored regions of the solar system	To explore fields and particles outside the ecliptic plane, in the vicinity of the heliopause, and in the interstellar medium	Third priority as a separate mission but first priority in the form of piggyback instrumentation on suitable planetary missions, such as Grand Tour
Synchronous Explorer	To provide a spacecraft devoted to science at synchronous altitude	Third priority in view of ESRO plans to orbit similar spacecraft. They would complement each other, however, through appropriate selection of longitudes

^a IMP Clusters L and M are designated as "Clusters A and B" in Chapter 1.

posited in varying amounts at all levels by excitation, dissociation, and ionization of neutral particles, and this energy creates a complex thermal structure which in turn determines the altitude distribution of the neutral particles. Energy also passes from the lower to upper atmosphere as waves, generating turbulence that appears to be widespread below 100-km altitude and is a major factor in the dynamics of the region.

The justifications for upper-atmosphere research are many. In terms of the science and technology involved, the gains will be large. We are now beginning to perceive the behavior of our atmosphere as a totality of interacting components; we have the technology and skills required to place this structure on a firm foundation. In terms of practical applications, the potential benefits of the program are also great. The problems of air pollution (including the photochemistry and dispersion of pollutants) and weather modification cannot be considered without taking the upper atmosphere into account, and to do this properly our existing knowledge must be increased. Many of our human activities are now recognized to inject potential contaminants into the upper atmosphere where residence times may be of the order of years. Because the upper atmosphere's total mass is so small, it is fragile: modification of the lower atmosphere, whether deliberate or inadvertent, could have effects on the upper atmosphere that we must be in a position to predict and avert.

Upper-atmosphere research has important applications for communications because the ionosphere remains an important factor in radio propagation. A complete understanding of the mechanisms underlying the production, movement, and destruction of atmospheric ionization is essential to full exploitation of the ionosphere as a means of communication. Even satellite communications, which use frequencies well above those normally thought to be subject to ionospheric influences, are in fact affected strongly by the presence of ionospheric irregularities.

Upper-atmosphere research is a field in which the challenges are many. The technology exists to meet many of these challenges, and a broad base of skilled scientific manpower can be tapped. The potential gains, both in our understanding of the environment and in our ability to modify it, are immense. The program that we propose will take us a long way toward the realization of these gains.

Recommended Program for Upper Atmosphere

Direct satellite probing of the upper atmosphere has been largely confined to altitudes above 300 km, because at lower altitudes atmospheric drag soon slows the satellite out of orbit. Yet the most important region of the

upper atmosphere from the point of view of absorption of solar energy in extreme ultraviolet (euv) wavelengths lies well below this level. *In situ* probing of the region below ~ 300 km has been confined to a few snapshots obtained with sounding rockets and a few short-term satellite experiments. The bulk of our knowledge of this lower region has been obtained by courageous extrapolations from direct knowledge of higher regions.

The technological advance represented by the Atmospheric Explorer (AE) series of satellites will allow us to probe this region for the first time in a satisfactory manner. These satellites have the propulsion capability required to maintain orbits with perigees as low as 130 km and to operate in circular orbits of adjustable altitude. The existing plan to launch these satellites in pairs, one in a near-equatorial and one in a near-polar orbit, will provide essentially continuous coverage both in latitude at given solar position and in local time around the equator.

Atmospheric Explorer constitutes the backbone of the proposed aeronomy program. Because a relatively unexplored region of the upper atmosphere will be studied by the presently approved satellites AE-C, -D, and -E, we prefer that the precise missions of the later satellites proposed for the series remain flexible for the present: should these missions be strongly influenced by the findings of the earlier satellites their primary aim will be the attainment of a comprehensive picture of the upper atmosphere as a vital link in the solar-terrestrial system.

The program proposed by this Working Group includes an Electrodynamic Explorer (EE) series of satellites. These are visualized as having the same powered capability for low-perigee and adjustable circular orbits as the AE satellites, but their chief mission will be to study the electrodynamic coupling between the ionosphere and the magnetosphere. This coupling arises from the strong anisotropy of electrical conductivity in the atmosphere caused by the earth's magnetic field. The geomagnetic field lines can be thought of as highly conducting "wires" embedded in ionized gas (plasma) which distribute electric fields generated at one level to all other levels. These "wires" are believed to dominate motions in the plasma and the growth and decay of plasma instabilities, but the validity of this concept and its consequences have been little studied experimentally.

It is essential to maintain an adequate sounding-rocket program to investigate the mesosphere and the portions of the lower thermosphere that will remain inaccessible even to the AE satellites. The mesosphere is the least well understood region of the atmosphere, partly because of the inherent difficulties associated with *in situ* probing and partly because of the great complexity of the processes occurring there. Advances in tech-

nology now afford an unparalleled opportunity to explore the constitution and dynamics of this vitally important region in which the coupling between the lower and upper atmospheres is most directly evident. An increased sounding-rocket program designed to fully exploit existing and developing technology in this area is strongly recommended. We further recommend that more attention be devoted to coordination of experiments and of rocket firings than has generally been the case in the past. In particular, coordination with the AE program should prove especially valuable in providing a view of the coupled mesosphere-thermosphere system.

Aeronomy has a requirement for monitoring the radiation from the solar disk in x-ray, euv, and uv wavelengths. Solar astronomers are uniquely qualified to perform these measurements, but the task has a relatively low priority in the exploratory Orbiting Solar Observatory program because its chief aim is the detailed study of the sun *per se* rather than its input to the earth's atmosphere. Although aeronomy requires only total-disk monitoring, which is much simpler than the high-spatial-resolution observations required by solar physics, the wavelengths of importance range from the near uv to x-ray wavelengths of about 1 Å. This entire band cannot be monitored continuously at high spectral resolution, and some kind of compromise is needed. For example, fairly broad bands—say, 1–10 Å and 10–100 Å—could be monitored in the x-ray portion of the spectrum, together with certain important lines in the euv and uv—for example, He I 584 Å, He II 304 Å, Lyman- β 1027 Å, and Lyman- α 1216 Å.

Agencies other than NASA presently have plans to launch satellites for solar monitoring. Examples are the Naval Research Laboratory's SOLRAD and the National Oceanic and Atmospheric Administration's GOES, which is intended primarily as an operational meteorological satellite but whose synchronous orbit will be ideal for solar monitoring. These agencies should be encouraged to monitor within the wavelength regions of concern, and any additional monitoring that appears to be necessary could be included as part of the mission of the proposed Solar-Terrestrial Probes if payload limitations allow.

SOLAR WIND AND MAGNETOSPHERE

The normal state of most of the matter in the solar system, the galaxy, and, very likely, the universe as a whole, is plasma. Many, if not most, astrophysical phenomena are governed by the principles of plasma physics, that is, the interaction between ionized gas and magnetic and

gravitational fields. The solar-terrestrial complex, though infinitesimally small compared with the total universe, shares these universal principles and offers insight into an astonishing variety of fundamental plasma processes that elsewhere occur on a cosmic scale.

It would be difficult, if not impossible, to attack the fundamental problems of cosmic plasmas in the laboratory. There is no way to scale spatial extension, temperature, and density to equivalent laboratory conditions. Observational conditions in space could never be matched by laboratory experimentation: space probes are of insignificant size compared with the physical systems under observation, and thus they negligibly perturb the effect to be measured.

We have, therefore, an excellent astrophysical plasma laboratory at our disposal in nearby space. Indeed, no man-made laboratory can parallel the particle-field environments of the earth and the sun as experimental regions for observing, *in situ*, fundamental astrophysical plasma processes in action. On some occasions we have even been able to alter the local conditions; and we can perform physical experiments in the classical sense.

Major advances have been made in recent years in the description and understanding of the interplanetary medium and the exterior portion of the geomagnetic field—the magnetosphere. We are approaching a quantitative understanding of the fundamental processes, some of them with far-reaching extrinsic impact. For instance, we have found the first direct evidence of large-scale magnetic trapping of energetic particles in the earth's environment and have evidence for trapped radiation elsewhere in the universe: in solar flares, around Jupiter, in interstellar space, in supernovae, in the Crab Nebula, and around neutron stars. We have found and are beginning to study quantitatively an acceleration process in the earth's particle-field environment that we believe to be of universal nature. The sequence of violent mechanisms that accompany the polar aurora—a sort of "lightning discharge" between the geomagnetic tail and the ionosphere, during which magnetic energy stored in the tail is suddenly converted into kinetic energy—may also be operative in solar flares and other cosmic explosions. We have found a correlation between the direction of the magnetic field in the sectors of the solar wind and the average polarity of the magnetic field on the solar surface and a definite pattern for active regions near the sector boundaries that could lead to more accurate long-term solar forecasting. Finally, on the basis of coordinated measurements of shock waves and other magnetic-field discontinuities in the solar wind, we are beginning to understand the physics of collisionless plasmas.

There are other reasons to study the earth's particle-field environment.

We know that up to about 1 percent of the energy of the solar wind striking the magnetosphere is transferred into the interior. As we come to understand the mechanisms by which this transfer occurs, we will be more able to predict the environmental factors that affect civilian and defense applications satellites. Similarly, perturbations of the particle-field environment caused by the coupling between the solar wind and the magnetosphere seriously interfere with communications systems and with nuclear detection systems. Better understanding of magnetospheric physics could lead to prediction and ultimately, perhaps, to control of some upper-atmosphere processes.

On the basis of the results of the past decade of exploration we are now able to identify the fundamental problems that must be attacked, and we are able to pinpoint the regions in space that need further study and to identify the instrumentation, orbits, and time correlation of launches to be recommended.

The complexity of the phenomena and their interrelations demands a clear separation of spatial and temporal effects in the experimental observations. For instance, it is necessary to determine whether a given variation detected in the interplanetary magnetic field represents a wave phenomenon propagating through the medium or whether it is a more-or-less static irregularity "frozen" into the solar wind and being convected past the spacecraft. In studying magnetospheric processes it is essential to know whether a given increase in particle flux is caused by local acceleration, or whether it represents the passage of a particle "cloud" that originated elsewhere and is drifting past the satellite. On the other hand, for a complete dynamical description it is not enough to obtain information on individual values of the relevant physical quantities: we must have simultaneous information on their spatial rate of change or gradient. Most information obtained to date has come from individual spacecraft measurements, independently planned, performed at isolated positions in space and time, and analyzed individually. Recent correlations of data from different spacecraft that happened to be conveniently positioned relative to each other at a given time—a rare occurrence—have proved this to be a most powerful tool for quantitative studies of dynamical processes.

There is only one unambiguous way to overcome the above difficulties: to perform simultaneous measurements with similar instrumentation at spatially different (but not too distant) positions.

Recommended Program for Solar Wind and Magnetosphere

We thus propose to base the program of new starts during the next decade on the concept of "mother-daughter" and "cluster" satellites, comple-

mented with other closely coordinated spacecraft positioned in the solar wind and the magnetosphere. To ensure proper planning and coordination of experiments and a more rational and efficient exploitation of data, we further recommend adoption of the investigation-team concept for all appropriate solar-terrestrial physics satellite and rocket experiments.

Recent proposals have shown encouraging signs of developing the team concept in which the broad outlines of the experiments are designed by a group of individuals with distinct but overlapping interests, and the entire body of data is made available to all members of the team for coordinated analysis. This approach is already being followed in the Atmospheric Explorer series. This Working Group strongly recommends that collaboration among individual experimenters be a criterion of the utmost importance in the selection of experiments for future spacecraft and that the data be made readily available to a wide base of potential outside users as quickly as possible. Where practical, the investigator team should include theorists as well as experimenters, and this team should have responsibility for both design of the experiments and joint analysis of the data.

We recommend that the major thrust of the experimental program during the next decade should focus on the study of (1) the solar-wind properties and dynamics free from the perturbing influence of the earth and (2) a comprehensive quantitative physical description of plasma processes in the magnetosphere. These goals are not independent of each other. They must be complemented with missions to as yet unexplored regions, such as to the solar wind at large distances from the sun and off the ecliptic, and to the outer magnetosphere at high latitudes. They must be accompanied by collateral measurements such as solar monitoring, cosmic-ray propagation and modulation, and thorough rocket and ground-based geophysical programs.

Solar-Wind Program

The measurements required to answer questions on solar-wind dynamics are greatly complicated by the energetic particles and the electromagnetic and electrostatic waves generated at or near the earth's bow shock which can frequently travel upstream and downstream in the solar wind. This perturbing influence must be minimized if the solar wind is to be studied as a stellar atmosphere and must be completely eliminated to obtain a correct analysis of low-energy solar and galactic cosmic-ray propagation and diffusion.

To accomplish these goals, we recommend that three Solar-Terrestrial Probe missions be launched at 3-year intervals. Each would be placed in heliocentric orbit within 10 million to 15 million km of the earth, with specifications similar to those given by NASA for the "space weather"

probes. Major emphasis should be placed on measurements of plasma distribution and composition, magnetic field and fluctuations, and cosmic rays, as well as on high-data-rate transmission. These measurements, undisturbed by the earth, when correlated with solar features observed from the ground, by OSO satellites and by the SOLRAD network, will make key contributions to the study of features of solar wind dynamics such as heat flow, determination of the degree to which the plasma particle distribution deviates from fluidlike equilibrium, charge and isotopic composition of heavy ions, sector field structure, discontinuities and irregularities, and waves. They will give important clues to solar activity and forecasting. Correlations with magnetospheric perturbations will yield information on the kind of boundary conditions and discontinuities to which the "plasma bag" of the magnetosphere responds most readily. In other words, the Solar Terrestrial Probes should define the nature and extent of solar-wind input to the magnetosphere.

Interplanetary spacecraft offer outstanding possibilities to investigate the plasma, field, and energetic particle properties of the solar system. An out-of-the-ecliptic planetary probe offers the opportunity to explore an unknown region of the solar wind with plasma probes, magnetometers, and cosmic-ray detectors. Knowledge of this region is germane to the general question of stellar atmospheres, in particular to the analysis of the problem of differential versus rigid rotation of solar surface features. Particle and field detectors on a Grand Tour mission, provided continuous transmission were ensured, would allow us to observe the outer boundary of the heliosphere where the solar wind interacts with interstellar gas and could provide the opportunity to probe directly the interstellar gas and low-energy galactic cosmic rays unperturbed by solar influences.

Magnetosphere Program

A comprehensive, quantitative study of magnetosphere dynamics requires (1) determination of the transfer of energy, momentum, and particles from the solar wind through the outer boundary into the magnetosphere; (2) determination of the cause-effect relations among events occurring during the magnetic substorm (time sequence and quantitative interrelations); and (3) study of the role of the ionosphere in control of the configuration of the electric field in the magnetosphere (particle convection and field-aligned currents) and its dual role as particle dump and particle supply.

This Working Group considers that Topic 2 should be the central motive of the magnetospheric program for the next decade. The substorm and associated auroral phenomena appear to represent a universal plasma acceleration process, and every aspect of these phenomena is in

principle open to direct, detailed probing. Work to date has done more to reveal the tremendous complexity of the phenomenon than to bring definitive understanding. Many of the basic mechanisms underlying the aurora take place in the mysterious "cusp" region of the outer magnetosphere where the plasma sheet of the geomagnetic tail merges with the dipole-like magnetic field, a region in which we have been unable, except on rare chance occasions, to separate temporal and spatial variations. The cluster concept will remove this restriction.

For the study of Topics 1 and 2 we recommend a team-operated program of particle-and-field measurements beginning with the mother-daughter system IMP KK' to explore the outer magnetosphere and its interfaces and followed by a series of clusters of four subsatellites in tetrahedral formation. Each cluster should be launched at approximately 3-year intervals, two in roughly circular equatorial orbit at ~ 8 earth radii to explore the inner edge of the plasma sheet and a third cluster placed at 10–20 earth radii for interface studies. The precise payloads should be responsive to the state of auroral theory at the time and should be designed to answer specific questions on the auroral acceleration mechanism. These measurements should be complemented by two small satellite missions, one to study the unexplored region of the neutral points and the other to investigate the plasmopause. During the latter part of the decade, a satellite in synchronous orbit to measure plasma, distributions of trapped and solar particles, and magnetic-field properties in the closed field-line region should complement cluster observations.

For the study of Topic 3, Electrodynamic Explorers in complement with rocket experiments (e.g., barium cloud injections and field-aligned current measurements) will furnish key data.

The program outlined above departs somewhat from the recommendations of the 1968 Space Science Board study, *Physics of the Earth in Space: A Program of Research 1968–1975*. We have not explicitly mentioned many important problems such as the composition (in particular the He/H ratio) of the radiation belts and its spatial and temporal variations, radial diffusion at low L -values, solar cosmic-ray entry into the magnetosphere, high-energy neutron albedo flux, or the artificial injection of matter to probe the magnetosphere. This does not imply that the importance of these topics has in any way decreased but simply reflects our view of the most pressing priorities in a period of increasing budgetary restrictions. Some of these outstanding problems can be attacked by suitable instrumentation on the new spacecraft proposed, and some may also be amenable to study through further analysis of existing data.

We anticipate that defense and operational space programs of other agencies will continue to carry out space research. Their contributions

would become critical to the future of space research were there to be a considerable reduction of new starts in the NASA program.

RECOMMENDED PROGRAM FOR SUPPORTING RESEARCH

Supporting research (i.e., research not tied to a specific spacecraft mission) is of the utmost importance to solar-terrestrial physics. This section is concerned with specific aspects of this supporting research that require emphasis during the next decade.

Data Analysis

Past spacecraft have produced large volumes of data which have remained unanalyzed mainly because of the lack of adequate manpower and funds. There is a particular need for more studies involving the joint use of data obtained by different experiments on a single satellite and of data from similar experiments on two or more satellites that happened to be in orbit simultaneously. This type of joint analysis has occasionally been carried out in the past and has yielded a high scientific return. It has been hindered by the principal investigator concept which has its own advantages but also favors analysis by relatively isolated small groups.

We strongly recommend that funds for data analysis in solar-terrestrial physics be increased, and that scientists in this field be encouraged to make extensive use of the bank of existing data. This will be particularly critical in the case of a low level of funding for new programs. For such an emergency, we also recommend that standby satellites in stable orbits be reactivated.

Theoretical Research

Theoretical studies are an essential component of solar-terrestrial physics, both to synthesize observational findings into a framework of understanding and to identify the specific questions toward which future experiments should be directed. Theory and experiment have traditionally gone hand in hand. In solar-terrestrial physics, one can cite cases in which theory is well ahead of experiment, and the immediate need is for an increased experimental program: the physics and chemistry of the mesosphere is a timely example. There are probably more cases, however, in which the opposite is true, where there is a pressing need for theoretical evaluation of experimental facts. An example is the aurora, in which the space

experiments of the past decade have presented us with a bewildering variety of facts which are yet to be rationalized into a theoretical framework.

Theoretical research is less glamorous, but also much less expensive, than experimental research. We strongly recommend that support for theoretical research be increased to a level adequate to meet the needs of solar-terrestrial physics in the next decade.

Sounding Rockets

Satellite experiments tend to give information on events occurring over a wide geographical range at given altitudes, and the corresponding necessary information on altitude variation at a given place and time can only be obtained from sounding rockets. Support for rockets has tended to decrease in recent years. We recommend that this tendency be reversed. The need for sounding rockets in aeronomy has been described in the Upper Atmosphere Program above and is emphasized here again: sounding rockets also have a role to play in magnetospheric physics (e.g., in measuring electric fields through barium ion releases) and in testing equipment designed for future spacecraft. An increase in funds allocated to rocket sounding for solar-terrestrial physics research is a high-priority item, especially in case of low-level funding of new programs. Close attention should be given to the global distribution of rocket facilities with a view to providing adequate coverage of all regions of interest.

Piggyback Experiments

Space missions planned for other programs often provide opportunities to carry instruments for solar-terrestrial studies. This is especially true of applications satellites in synchronous orbit, space stations, and planetary missions such as the Grand Tour. We strongly recommend that adequate funding be made available to take advantage of such opportunities. In the event of low-level funding of new starts, piggyback experiments will provide a relatively inexpensive means of attaining at least some of the objectives of the field.

Ground-Based Research

Ground-based research has always been recognized as a vital feature of solar-terrestrial physics. Although much of this research is carried out by agencies other than NASA, we recommend that adequate support be provided in those areas that bear directly on rocket and spacecraft missions (e.g., solar monitoring and geophysical indices).

International Cooperation

Cooperation with other countries is becoming an increasingly important aspect of space science in general and of solar-terrestrial physics in particular. Among specific space missions planned by other countries are the European Space Research Organization's (ESRO's) HEOS-A2 mission to explore the outer magnetosphere at high latitudes, the proposed ESRO synchronous satellite, and the joint German-U.S. HELIOS mission to probe the inner solar system to within 0.3 AU of the sun. Recently, IUCSTP and COSPAR (on both of which the international astronomical and geophysical bodies are represented) have also proposed an internationally coordinated effort during the coming decade to study the magnetosphere through satellite and ground-based measurements made simultaneously at different locations. We urge that high priority be given to active participation in these and other international projects. The potential gains in both scientific knowledge and international understanding are too great to be dismissed.

8 Earth Environmental Sciences

This chapter is concerned with the use of space techniques to study the terrestrial environment, terrestrial resources, and the biosphere in the atmospheres, oceans, land, and solid earth. Observing the earth from space is complicated because the scales of the phenomena that we wish to examine range from microscopic to global. Moreover, the nature of the studies involved ranges from the highly analytic, for certain problems of the physics of the atmosphere, oceans, and solid earth, to the necessarily descriptive for the higher level of organization of the ecosystems and their environmental interactions. An important object of study is the role of man in these interactions. Man's practical dependence on the earth for resources and his need for a benign environment are the main drives behind the study of the earth and greatly influence the development of the relevant scientific disciplines. For convenience—albeit somewhat arbitrarily—we have divided these interacting disciplines into six topics: meteorology, oceanography, hydrology, the biosphere, cultural features (urbanization and the effects of technology), and solid earth (geophysics and geology).

This chapter is to an appreciable extent the successor to the more extensive report, *Useful Applications of Earth-Oriented Satellites*, carried out by the National Academy of Sciences in 1969. In addition to updating some aspects of the 1969 study, this chapter gives greater emphasis to the scientific potential of earth-oriented satellites. Other previous studies applicable to this chapter are: *Plan for U.S. Participation in the Global*

Members of this Working Group were W. Kaula, *Chairman*; A. Anderson, R. Arnold, P. Bock, F. Doyle, G. Ewing, W. Fischer, R. Fleagle, J. Hobbs, F. Horton, P. Johnson, A. Katz, R. Porter, W. Rubey, J. Shay, J. Smagorinsky, W. von Arx, and G. Zissis.

Atmospheric Research Program, Remote Sensing with Special Reference to Agriculture and Forestry, Resources and Man, and The Terrestrial Environment: Solid-Earth and Ocean Physics.

METEOROLOGY

Meteorology is concerned with the dynamics of the earth's atmosphere, especially that of the lowest portion—the troposphere—where most of man's activities are carried out and in which there occur clouds, storms, precipitation, and other phenomena known as weather. The application of this science is primarily to weather forecasting; however, there is now convincing evidence that under certain conditions the weather can be beneficially modified to augment rainfall or to moderate severe storms. Meteorology is also important to improved control of air pollution. The benefits to be derived from the current revolution in meteorology (as a result of techniques to gather and process weather data quickly and frequently on a global scale) are so numerous and far-reaching that there should be no question about continuing public support for space research devoted to meteorology.

The hydrodynamic equations that describe the motion of the atmosphere are well known and have been applied to the construction of models of the general circulation of the atmosphere, and to theoretical studies of the growth, motion, and decay of localized storms. However, the level of understanding represented by these models is far from satisfactory, and many interesting challenges remain. Especially important is the need to understand better the transfer of momentum, energy, and moisture at sea and land surfaces, at the boundaries between distinct air masses in the troposphere, and between the troposphere, stratosphere, and mesosphere. Absorption and reflection of solar energy and of the low-temperature radiant energy from the earth by atmospheric contaminants and clouds are also important but poorly known on a global scale. Existing models of atmospheric circulation must be improved, new methods of modeling applicable to long-range predictions (climatic time scale) must be devised, and better observational means to test these models against observations must be developed and implemented.

Meteorology is related to physical oceanography and aeronomy because the oceans and the total atmosphere must ultimately be regarded as one coupled system. It is similarly related to studies of terrestrial hydrology and the biosphere, both because of the coupling at boundaries and because of commonality in sensor technology. Inasmuch as the earth is only one of several planets that possess atmospheres, meteorology

might properly be considered a subdiscipline in the study of planetary atmospheres. Finally, there may be useful scientific analogies between meteorology and the study of stellar "atmospheres," including some aspects of solar physics.

OCEANOGRAPHY

Oceanography is concerned with various aspects of seawater: its motions and chemical constituents; its physical properties and behavior; its relationships to the solid earth, the atmosphere, and living organisms of all kinds; its economic and technical potentialities; and its role as a part of the earth's outer covering. Problems of oceanography to which space techniques can be applied are:

1. The general circulation of the oceans: to account for the observed currents, temperature, and salinity; to understand the nature of turbulent transport; to infer the deep return currents; and to describe the role played by the atmosphere in driving their circulation. Knowledge of sea-level variations with respect to the geoid, such as a satellite altimeter might obtain, would provide a surface of known pressure heretofore lacking in interpretations of the oceanic circulation. Measurement of surface temperature would delimit more accurately the spatial and temporal variations of such features as western boundary currents and cold-water upwellings. Tracking of free-floating buoys could improve the description of large-scale turbulence.

2. The tides raised by the sun and moon: Altimetry could describe their interactions with the shape of the ocean basin and help to infer the nature of the associated energy dissipations.

3. Wave structure and its relation to the atmosphere: Radar scatterometry from satellites could measure the wave spectrum (the distribution of power among different wavelengths) over a wide range of conditions.

4. The distribution of life in the oceans, in relation to such phenomena as temperature and current patterns: Satellite-borne spectrometry in the appropriate wavebands could measure the amount of chlorophyll, which is directly proportional to the amount of phytoplankton—the lowest level in the trophic pyramid. At the highest trophic level, migrations of animals that surface and that are large enough to carry radio beacons (whales, turtles) could be tracked by satellite.

Satellites would afford oceanography a much better sampling of measurements relative to some of its characteristic length and time scales and act as a reconnaissance tool by locating anomalous features for surface vessels to investigate.

HYDROLOGY

Hydrology is the science of water, particularly its interaction with the land: storage and transport beneath and at the surface and gain and loss of water to and from the atmosphere and sea. The primary problem is to understand the hydrological cycle—the various paths taken by water in relation to the land. This cycle is one in which the actions of man are having an increasingly significant effect, and hence hydrology has a strong applicatory emphasis.

Remote-sensing techniques from satellite or aircraft in combination with communications and computer capabilities offer new possibilities beyond the conventional methods presently available. Multispectral imagery could be applied to estimate: (1) the extent of snow and ice cover from its pattern relative to topography and vegetation; (2) changes in, for example, shoreline, pollution, and oil slicks of or around lakes, rivers, estuaries, and coastal waters; and (3) variation in vegetation and other land cover affecting evapotranspiration and runoff. Satellite interrogation of remote surface gauges could relay data on the evolution of floods. Eventually, infrared remote sensing may contribute to the estimation of soil moisture.

THE BIOSPHERE

Biosphere connotes all organisms and the habitats and environments with which they interact over the globe. Ecology is concerned with understanding the interaction of organisms with each other and with the environment. A large number of specialized disciplines are concerned with those ecological systems (ecosystems) managed with increasing intensity by man for water, minerals, power, transportation, residence, recreation, waste disposal, and the like. In recent years, man's demands on earth resources have been estimated to have increased at the rate of five to six percent per year. This increase has resulted in noticeable deterioration of the environment with consequent social effects.

The general understanding of interactions among organisms and the influence of environment on these interactions has advanced from qualitative description to analytical testing of relationships and to the development of models that have some predictive power. The principal problems outstanding can be put in three groups:

1. What is the relationship between the function and structure of ecosystems: what are the driving forces and processes in each organism

and in each element of the land, air, and water environment, and what are the mechanisms for interchanges between these organisms and elements in various ecosystems?

2. What is the existing and potential productivity of ecosystem components in space and time, and how do they respond to system stresses; what is the proper base for resource management; what are the consequences of optimizing for a particular combination of products; what are the controlling variables modulating matter and energy transfer?

3. How do ecosystems change; what regulates their stability; what factors determine the paths and rates of ecological succession; what is the impact of particular technologies on particular ecosystems?

Successful application of remote sensing to vegetative ecosystems depends mainly on marked differences in spectral reflectance and radiance of ecosystem components in spectral regions from 0.32 to 14 μm . The most marked property is the high reflectance of chlorophyll-bearing plant organisms in the near infrared (0.7–1.5 μm); it is easy to distinguish between green living plants, dead or dying plants, soil, and water. Differences between plant species appear largely as differences in power reflected at various wavelengths over a wide range of the spectrum, rather than in localized details in the shape of the spectral curves.

In recent years, there has been a rapid development of remote sensing from aircraft. There thus exists the technical base to use satellites to increase greatly the sampling in time and space of both natural and managed ecosystems. This sampling will allow the extrapolation of data obtained at intensive research sites to much larger units of the landscape. The improved knowledge of distribution and phenology of vegetation relative to climate, topography, soils, and other factors will contribute greatly to the advance of ecological science. Remote-sensing techniques also apply to problems of practical management in agriculture and forestry such as predicting crop yields, controlling pests, assessing soil quality, and planning plantings and harvests. The most effective program will certainly be a combination of space and aircraft techniques.

CULTURAL FEATURES: URBANIZATION AND THE EFFECTS OF TECHNOLOGY

The outstanding problem of our society is the rapid growth of man's influence on his environment. These influences are most drastic in conurbations, which range in character from "megalopolises"—complexes of old cores in belts of suburban sprawl characteristic of highly developed

countries—to a phenomenon unique to our century, the increasing concentrations of population in underdeveloped countries since the introduction of public-health measures. Objects of scientific study under this heading include any development of human activities relative to topography, water, resources, and the like.

Urban researchers have been handicapped for decades by lack of adequate pertinent cross-sectioned and time-series data. Aircraft photography is currently being applied to some of these problems. At present, better than 10-m resolution is thought necessary for such problems as traffic flow and housing quality, but increased research on the potentialities of lower-resolution imaging is urgently needed. At 100-m resolution, land-use patterns relative to transportation systems, water, and topography could be studied, and a reasonable evaluation of air, water, and land interrelationships should be possible. Observations repeated at the rate of one to six times a year are appropriate for the higher resolution data. Here, techniques that merely indicate locations where change has occurred would be of great practical value. Remote sensing by aircraft will probably be the method of choice for urban and regional problems in the United States because of resolution requirements. More research is needed to determine how much satellites may contribute to urban studies. The potentialities are obviously greatest on an international scale, where satellites would enable application of more remote-sensing techniques to a variety of cultures.

SOLID EARTH: GEOPHYSICS AND GEOLOGY

This group of disciplines can be conveniently divided into three subjects: (1) the evolution of the earth's surface, as observed in the geologic record; (2) the dynamic processes within the solid earth; and (3) the interaction of the solid with the fluid parts of the earth. These subjects correspond roughly to distinct but related scientific communities.

The evolution of the earth's geologic surface is an essentially descriptive subject because of its complexity. Space techniques might apply to two aspects: (1) The definition by imagery of tectonic lineaments, fractures, and zones of weakness. (Often these lineaments are so diffuse that detection by ground observations is a difficult task.) (2) Mapping of minerals by multispectral space observations utilizing variations in vegetation and soil moisture which often reflect the character of the underlying bedrock. Both the imaging and spectral techniques would have reconnaissance and interpretative value for geologic mapping, particularly in remote areas.

Study of motions in the solid earth has developed rapidly since 1963. It now appears that a large part of recent tectonics can be interpreted in terms of rotations with respect to each other of six major rigid lithospheric plates and several smaller ones. Considerable progress has been made in understanding the nature of the interactions at the boundaries of the plates and in the theory of the thermal convection of which plate tectonics is the surface manifestation. The outstanding problem now is to understand the nature of mantle convection, the location of the heat sources that drive it, their interaction with mantle rheology and chemical processes, and the causes for changes in the tectonic pattern with time. Space techniques can help with this problem in essentially two ways: (1) by measuring the variations in space and time of the plate motions, which would require location accuracies on the order of ± 2 cm in a program lasting decades and (2) by measuring the spatial variations in the gravitational field either through satellite orbit perturbations or, over the oceans, by the geoidal undulations. The gravitational field is the principal indicator of heterogeneities important in mantle convection. The greatest improvement in knowledge of gravity variations seems feasible by satellite-to-satellite tracking. This technique would enable an improvement in resolution of the field from a half-wavelength of 1200 km to one of 250 km and an eventual capability to measure variations with half-wavelengths of 100 km. In this manner, relations of the gravity field to the tectonic pattern, just now perceptible, should become much clearer.

The interaction of the solid earth with the oceans and the atmosphere is reflected by variations in the rotation rate and wobbles of the earth in the direction of its rotation axis. The interaction of the solid earth with the liquid core also affects its rotation and bears on the hydromagnetic dynamo by which the earth's magnetic field is generated. These studies could be aided by: (1) more accurate locations, such as mentioned in connection with the monitoring of plate motions, and (2) more accurate measurement of the magnetic field. Although the nature of the geodynamo is mainly a theoretical problem, more accurate knowledge of the magnetic field should provide a better indication of the characteristic length scales of the core convective system.

PROGRAM RECOMMENDATIONS

The space techniques that are being applied to the six areas described above are of two kinds: those using the satellite as a platform from which to sense electromagnetic radiation emitted or reflected from the earth and those using it as a point that can be located accurately over great

ranges in space and time. The first category can be further subdivided according to uses that require (a) relatively low resolution (poorer than 1 km) and high repetition rate (twice a day or more often) and those that require (b) relatively high resolution (better than 1 km) and low repetition rate (once a day or less often).

These combinations of capability with use have resulted in a tripartite structuring of the NASA earth observation program:

1. Meteorological satellites, primarily to observe variations in the atmosphere but also to provide oceanographic data (e.g., sea state and surface temperature) and hydrological data (e.g., precipitation and snow coverage).

2. Earth resources survey satellites, primarily for multispectral sensing and mapping of surface features, with applications to hydrology, oceanography, agriculture, ecology, forestry, geography, geology, urban studies, and other resources.

3. Earth physics satellites, to determine spatial and temporal variations in the geometry, gravity, and magnetic fields of the earth, with application primarily to the study of the solid earth and oceans.

The types of satellites in an earth-oriented program depend not only on their orbits and the nature of their measurements but also on their stage of development. These stages are (1) research—to test new sensors and theoretical models, for example, and (2) prototype—to test operational systems that will be operated by another agency. These satellite types are shown in Table 6.

The apportionment of funds to any research satellite given below (except SATS) assumes their purchase in pairs, so as to reduce the nonrecurring costs per satellite. These costs are also those appropriate to the testing of significantly new instrumentation, ground as well as satellite; moderate modifications would be less costly. The costs include the data analysis, which is necessary to prove the system through to its scientific results. Additional analysis may be funded by other agencies. The SATS is a Scout-launched spacecraft requiring only a 1–2-year lead time, designed to perform experiments that arise too quickly to accommodate in the 3–4-year lead time of the large satellites. The SATS also is applicable to uses that require a special orbit for one or two experiments.

At any level of effort, a properly balanced earth-observation program must contain significant supporting research and technology (SR&T), mainly for research in data interpretation and analysis, and a sizable aircraft program, primarily for higher-resolution data to supplement satellite measurements and secondarily to test instrumentation. The Working

TABLE 6 Earth-Oriented Satellites ^a

Program and Type		NASA Nomenclature	
<i>Meteorology Program</i>			
Research			
Observatory, sun-synchronous	EOS	(Earth Observations Satellite)	
Observatory, geosynchronous	SEOS	(Synchronous Earth Observations Satellite)	
Small	SATS	(Small Applications Technology Satellite)	
Prototype			
Sun-synchronous	SMS	(Small Meteorological Satellite)	
Geosynchronous	TIROS		
<i>Earth Resources Program</i>			
Research			
Observatory, sun-synchronous	ERTS, EOS	(Earth Resources Technology Satellite)	
Oceanographic	ERTS, EOS		
Recovered	ERTS		
Observatory, geosynchronous	SEOS		
Small	SATS		
Prototype			
Sun-synchronous	ERS		
<i>Earth Physics Program</i>			
Research			
Altimetric	EPS	(Earth Physics Satellite)	
Gravimetric and magnetometric	SATS		

^a Also used for earth observations are ATS (Applications Technology Satellite) of the NASA Communication and Navigation program and Skylab and future vehicles of the manned space-flight program.

Group believes, in fact, that these efforts should be increased moderately relative to satellite projects. The distributions of effort considered appropriate at the different funding levels are given in Table 7. Table 7 is based on cost estimates of the satellite types in Table 6 furnished by NASA. Under "Flight Projects" these costs include the data analysis necessary to prove the system through to its scientific results.

The Working Group recommends the level 1 program as a sound, balanced effort which is an efficient response to the urgencies of the problems to which it applies. The effects of the lower levels are discussed below.

In addition to the work funded by NASA, research in support of sensor-signature understanding, sensor-signature research, and programs of earth-resources data analysis and dissemination funded by other agencies should total roughly twice the SR&T dollar support indicated by Table 7.

TABLE 7 Recommended Distribution of NASA Earth-Oriented Budgets

	Level 1 (\$235M/yr)	Level 2 (\$160M/yr)	Level 3 (\$105M/yr)	Level 4 (\$50M/yr)
<i>Meteorology</i>	35%	40%	44%	56%
Flight projects	22%	23%	24%	36%
SR&T	10%	13%	15%	12%
Other	3%	4%	5%	8%
<i>Earth Resources</i>	52%	45%	41%	40%
Flight projects	35%	26%	16%	18%
Aircraft surveys	11%	11%	14%	16%
SR&T	6%	8%	11%	6%
<i>Earth Physics</i>	13%	15%	15%	4%
Flight projects	6%	9%	11%	2%
SR&T including tracking	7%	6%	4%	2%
TOTAL	100%	100%	100%	100%

The recommended satellite programs corresponding to the four funding levels are given in Table 8 in terms of launches in the periods 1972–1973, 1974–1976, and 1977–1980. The launches for 1972–1973 are all approved projects *except* the recovered observatory. At levels 3 and 4, it is impossible to avoid a pronounced funding peak in fiscal year 1972 without costly cancellation of flight projects in progress.

DISCUSSION OF PROGRAMS

Meteorology

At present, functioning meteorological satellites include attitude-stabilized, sun-synchronous satellites of about 13 orbits/day (NIMBUS for research, TIROS as a prototype operational satellite, and ITOS, the NOAA operational satellite). The geosynchronous ATS satellites are also being used for meteorological purposes. All these satellites produce visual images of cloud formations, the sun-synchronous covering all areas at least once a day and the geosynchronous covering the sunlit portion of an area about 6600 nautical miles in diameter approximately once every 20 min. The sun-synchronous satellites also produce daytime and nighttime infrared images of clouds. An important recent advance is the NIMBUS infrared instrumentation for global remote sounding of atmospheric temperature and humidity; the inferred temperature profiles have already

TABLE 8 Recommended Earth-Oriented Satellite Programs

Time Period: ^b	Level 1, \$235M/yr ^a			Level 2, \$160M/yr ^a			Level 3, \$105M/yr ^a			Level 4, \$50M/yr ^a		
	2-3	4-6	7-0	2-3	4-6	7-0	2-3	4-6	7-0	2-3	4-6	7-0
<i>Meteorology</i>												
Obs., sun-synch.	2	2	2	2	1	2	2	$\frac{1}{2}^c$	1 ^c	1	$\frac{1}{2}^c$	$\frac{1}{2}^c$
Obs., geosynch.		1	2			$\frac{1}{2}^a$						
Small		2	2		2	2			2		1	2
Prototype, sun-synch.		1	1		1	1		1				
Prototype, geosynch.	2			2			2			2		
<i>Earth Resources</i>												
Obs., sun-synch.	2	3	5	2	3 ^c	2	2	$\frac{1}{2}^c$	1 ^c	1	$\frac{1}{2}^c$	$\frac{1}{2}^c$
Obs., oceanographic		2			[1] ^e							
Obs., recovered	2	[2] ^f		[2] ^e								
Obs., geosynch.		1	2			$\frac{1}{2}^d$						
Small		1	2			2		1				
Prototype, sun-synch.		[1] ^f	4			2						
<i>Earth Physics</i>												
Altimetric	1 ^g	1	1	1 ^g	1	2	1 ^g	1	2			
Gravimetric and magnetometric		1	3			2						1

^a Not including launch costs.

^b 2-3 is launch in CY 1972-1973 (none scheduled for 1971); 4-6 in 1974-1976; 7-0 in 1977-1980.

^c At levels 3 and 4, observatory satellites would be shared between meteorology and earth resources.

^d At level 2, a geosynchronous satellite would be shared between meteorology and earth resources.

^e One land-directed observatory might be replaced by two recovered satellites, or one might be replaced by an oceanographic observatory.

^f Not included in the funding level, but desirable if finances permit.

^g The altimetric satellite for 1972 only in GEOS.

been used in operational weather forecasting. The NIMBUS satellites scheduled for launch in 1972 and 1973 will continue the development of these infrared radiometers and spectrometers and will incorporate microwave techniques to measure atmospheric water (liquid and vapor) as well as temperature below the cloud cover. Other new capabilities will include real-time relay of data and pictures and satellite-to-satellite tracking (in conjunction with ATS); improved system for balloon tracking and data collection; an infrared limb-radiance experiment to measure stratospheric temperature, water vapor, and ozone; and more accurate monitoring of the earth's radiation budget.

The rapid progress in sensor technology and in effective use of satellites for meteorological research justifies the continued launching every three years of a pair of large, sun-synchronous experimental satellites similar in capability to NIMBUS, plus one of the shorter-lead-time SATS every second year. (After 1973, the requirement for NIMBUS-sized satellites is expected to be fulfilled by EOS, which would be used both for meteorology and earth resources.) This rate is shown in the level 1 program, which also includes one TIROS every third year to develop further the more valuable techniques for operational use. At level 2, the experimental capability beyond 1973 is cut 25 percent, and at level 3, another 50 percent. The SMS satellites will be prototypes for the operational NOAA GOES satellites at geosynchronous altitude, which will be the first to have an infrared imaging capability in this orbit. They also constitute an important international commitment to the Global Atmospheric Research Program (GARP), which is well described in the NAS report, *Plan for U.S. Participation in the Global Atmospheric Research Program*. We recommend their retention at all funding levels.

It is manifestly desirable to develop further meteorological instrumentation that takes advantage of the coverage provided by the geosynchronous orbit. We have recommended that this capability be provided by the launch of one SEOS every second year, beginning with 1975 at level 1 or by at least one launch in 1977 at level 2. At lower budgetary levels, such experimentation would have to depend on the availability of space in ATS satellites devoted primarily to communications technology.

There is a requirement for long-duration accurate monitoring of the earth's radiant energy budget, in particular of the total magnitude of the incoming solar radiation. An experiment of this kind is included in the 1973 NIMBUS. In addition, a suitable instrument package should be designed for use in geosynchronous satellites, preferably of an operational type, so that the measurements could be continued for at least one full solar cycle. Satellite techniques to measure the anticipated increases in

particulate matter in the stratosphere should also be developed. A possible technique is backscattering of laser beams.

The supporting, or "level of effort," part of the meteorological program includes the normal SR&T; a continuing rocket-sounding program for high stratospheric and mesospheric investigations; a small amount of continuing support by NASA for the ITOS operational satellite program; development of meteorological experiments for manned spaceflight missions, data analysis, and numerical modeling; and support for the international GARP program, including the 1973–1974 Tropical Experiment and the later Global Experiment. We have recommended a substantial increase in SR&T and in the data analysis and modeling activity at levels 1 and 2.

The lead agency for meteorological data management and processing is the National Oceanic and Atmospheric Administration. However, effective use of NASA's satellite program requires an expansion of data processing and utilization both within and outside NASA; specifically:

1. Long-term support to some additional universities for meteorological research and education directed toward utilization of satellite data;
2. Prompt mobilization of a competent group with responsibility to plan and execute the management and processing of data for the GARP experiments;
3. Development of a strong numerical modeling activity primarily concerned with simulation experiments for the design of a global meteorological observation system to which satellite data can contribute in an optimal way.

Earth Resources

The first ERTS satellites in 1972 and 1973 represent the culmination of several years effort by NASA in sensor development, testing in aircraft, and signature analysis. The two approved ERTS satellites cover three spectral regions with return-beam vidicon cameras, carry four- or five-channel multispectral scanners, and can collect data from surface sensors. To provide data for the analysis capability that has been established, we recommend retention of both these satellites through level 3 and of the 1972 launch even at level 4; information for planning should be obtained as soon as possible.

To build a time series of the phenomena observed, it is desirable to have a uniform annual rate of launch. Level 1 would maintain this rate for both prototype and research satellites in the 1976–1980 period. Level 2 maintains it by alternating prototype ERS satellites with research

EOS satellites. Level 1 also includes ERTS satellites from which film is recovered. It is desirable to have both the film-recovery ERTS and the image-telemetry ERTS satellites flown at the same time in order to obtain coverage of the same areas simultaneously. Recovered film from Skylab A will also provide partial coverage.

We also recommend that ERTS satellites dedicated to oceanography be launched in 1974–1975. These satellites must differ significantly from earth-resources satellites directed toward land coverage because: (1) to see into the water the mean wavelength of the sensor must be more in the blue portion of the spectrum and the viewing and sun angles must be larger; and (2) to overcome atmospheric backscatter the sensor bandwidths must be narrower.

With funding lower than level 1, it is impossible to carry on full programs of both land-directed and ocean-directed observatory satellites. For the earlier part of the decade, the land-directed projects should have priority because of greater readiness and a larger community of users of remote-sensing. For the latter part of the decade, it is not clear whether (a) the land-directed program should be given priority because the foreseeable increases in productivity of the land are greater than the oceans, or (b) whether an ocean-directed program should be given priority because the ocean is inherently more appropriate for satellite techniques.

At level 1, we propose geosynchronous earth resources survey satellites in 1976–1980 to observe diurnal variations in vegetation and soils; it is impossible to make these observations from sun-synchronous satellites. Because such a satellite would inherently require both high resolution and high repetition rate, the observations must necessarily be highly selective. With funding lower than level 1, the more elaborate sensor systems necessary for a geosynchronous earth resources satellite could not be developed.

At lower levels the cuts in earth-resources programs (such as the omission of prototype satellites) are greater than in meteorology, because the lower levels could result only from diminution in concern about the problems to which the resource survey satellites apply. Hence other factors such as preparedness for data analysis and theoretical modeling would have greater weight than practical urgency. However, at the lower levels we recommend an appreciably greater ratio of aircraft work and SR&T to spaceflight projects in order to continue sensor-signature research, systematic acquisition of data, and data analysis.

An essential component of an earth-resources survey is aircraft work, primarily to make surveys at greater resolution than attainable by spacecraft and secondarily to test sensors. The programmed NASA budget of \$11 million per year is estimated to support the operation of three aircraft

to cover five test sites totaling about 0.75×10^6 km² twelve times a year, plus ten test sites totaling about 0.80×10^6 km² five times a year. We recommend that this capability be enhanced at all levels down to level 3, with primary emphasis on the setting up of prototypes of regional operational flight programs. The option should always be maintained, of course, of shifting emphasis from spacecraft to aircraft techniques after analysis of ERTS results. This expansion of the aircraft program is particularly urgent with respect to the problems of urbanization and technological impact on the environment.

Specific recommendations which are not reflected in the program levels but which pertain more to the manner of implementation of the programs are the following:

Education and Manpower

To obtain the people needed to process, analyze, and disseminate effectively the data generated by the earth-observation systems, both space and airborne, we identify four requirements: (1) more orientation toward physics in undergraduate bioscience education; (2) increased graduate education in ecology and related life sciences disciplines; (3) formulation of procedures and establishment of training programs for a large number of interpretative technicians; and (4) training of supervisors for these technicians.

Data Analysis and Dissemination Techniques

It is the consensus of this Working Group that the greatest technical obstacle to effective use of earth-observation programs is the gap between the raw data as produced by NASA—imagery and magnetic tapes—and the end uses, both scientific and applicatory. Aerial photointerpretation is used in varying degrees in many industries, government agencies, and universities. However, the majority of these potential users are not prepared to accept now the much greater quantity of lower-resolution imagery which the earth-resources program will produce, let alone the spectral scanning and other data more appropriately transmitted on magnetic tapes.

We therefore recommend that the seed effort which NASA has already made be brought toward fruition by the establishment of several regional centers for remote-sensing data analysis. These centers would carry on research and development and assist local users in the application of remote-sensing data to their problems. They would also develop techniques for the operational data analysis. When operational data-analysis centers are established, they should be located in the vicinity of the regional R&D centers.

Sensor-Signature Research

Research into the fundamental physical properties affecting the response of vegetation and earth surface materials to the entire range of electromagnetic radiation should continue. Ecology, forestry, agriculture, and certain engineering fields could benefit greatly from spectral-signature research in soils sciences (pedology). In our opinion, scientists in this field have not been as active in sensor-signature research as those in some of the other fields. Measurement of soil types, subsoil characteristics, and moisture contents are all important for ecological research. NASA should vigorously pursue research to establish correlations with sensor signatures that will yield information on these phenomena.

Coordination with Other Observational Programs

This Working Group approves NASA plans for expanded aircraft survey programs. However, in some instances greater benefit could be realized by adjusting the flight schedules and surveys to include regions that are the object of intensive ecological studies under other programs. Two examples are the International Biological Program biomes and the Atomic Energy Commission sites.

Study of Social and Political Implications

There is a real possibility that an earth-resources survey could be used to exploit, rather than to aid, mankind by inadvertance, if not by intent. There are several potential reactions to this possibility, as well as other problematical political aspects among the many nations of the world. Hence, in addition to economic studies, NASA should support studies by appropriate institutions on these social, political, and legal aspects of earth-oriented uses of satellites.

Earth Physics

GEOS-C, to be launched in 1972, is similar to previous GEOS satellites in that it has a flashing light, laser retroreflectors, and various tracking transponders. It is dissimilar in that it has a low inclination, because of its primary dedication to gravitational rather than geometric purposes, and carries a radar altimeter accurate to ± 1 or 2 m, which will improve knowledge of the gravity field significantly in the one fourth of the world where it is most poorly determined by satellite orbit perturbations. GEOS-C will also have the forerunner of the more accurate oceanographic altimeter.

The most significant increase in knowledge of the gravity field would come by accurate tracking of a low-altitude, polar-inclination, drag-free

satellite from distant satellites. The distant satellites could be ATS's or other geosynchronous satellites. To obtain global coverage, this tracking would have to be performed by geosynchronous satellites at a minimum of four longitudes (not necessarily in the same time period if the close satellite has a long enough lifetime and a restartable drag compensation system). We propose an early very close "gravitational" satellite at levels 1-3, but at level 4 it is slipped from 1974 to 1977. If possible these satellites should also carry magnetometers to obtain more detailed global measurements of the magnetic field.

Improvement of radar altimetry to ± 10 cm is believed to be feasible by the late 1970's. We propose a pair of such satellites at altitudes on the order of 1000 km and inclinations of 70-80° (deliberately non-sun-synchronous, to measure tidal oscillations) at levels 1, 2, and 3, primarily for oceanographic and secondarily for solid-earth physics purposes. These satellites should be drag-free and tracked from geosynchronous satellites. They should also carry sea-surface temperature and spectrophotometric sensors if these do not interfere with the altimetry which, presumably, would require a rather large antenna to attain the ± 10 -cm accuracy.

We propose low-altitude gravimetry and magnetometry satellites at all levels to obtain accurate information on the gravitational field which is a necessary supplement to the geometric measure of sea level. At levels 1 and 2 several such satellites at various inclinations are recommended to measure the gravitational field with greater detail and accuracy, to measure variations in time of the geomagnetic field, and, most important, to take advantage of improvements in satellite-to-satellite tracking and ion propulsion in the late 1970's.

Other activities recommended which are not primarily functions of the flight projects listed in Table 7 are (1) vertical radar scatterometry; (2) tracking free-floating buoys; (3) data collection from surface sensors; and (4) very-long-baseline interferometry (VLBI) and laser ranging from the surface to distant satellites and the moon. The scatterometry is necessary to define the nature of wave structure well enough to assure a meaningful definition of the ± 10 -cm level; it should ideally be done on an oceanographic or meteorological satellite preceding the altimetry satellite. A capability of tracking 50 to 500 free-floating buoys, both surface and "pop-up" types, might best be incorporated in the altimetry satellite, because it will have the most accurately determined orbit of satellites at low enough altitude to be within range of relatively inexpensive low-power beacons. This capability could also be incorporated in a navigation satellite or sun-synchronous meteorological satellite. Requirements for data collection do not appear significantly different from those planned for satellites such as ERTS and SMS, except for the requirement that about

50 seismic and tidal stations should have override capability via geosynchronous communications satellites for tsunami warning.

To monitor tectonic plate motions, polar wobble, and rotational variations, tracking of distant satellites and the moon (as well as altimetric satellites) by VLBI or laser ranging would absorb several million dollars a year in the late 1970's at level 1 because it would require development of instruments aspiring to ± 2 -cm accuracy and their deployment at about 20 stations around the world. Ideally, the distant satellites would be a widely spaced array at altitudes of 5000 to 10,000 km, an elaboration which we do not see feasible even at level 1 before the 1980's. Hence we propose the use of geosynchronous satellites for this purpose. At level 2, the globally distributed tracking stations would number about six, enough to monitor polar wobble and rotation, but only testing a few examples of plate motion. At lower levels, laser ranging of ± 10 -cm accuracy should still be maintained at a few sites to monitor rotation and wobble and to determine satellite orbits accurately.

USE OF MAN IN SPACE

Earth-oriented observations, in common with other observational uses of space, can utilize manned spacecraft for heavy instrumentation that requires special attention and maintenance. One example is high-resolution spectroscopy.

This Working Group concentrated its discussions on those uses that are peculiar to earth observations, i.e., on whether a trained observer with appropriate optical aids can obtain information about weather or other phenomena which cannot be derived from telemetered or recovered imagery. It seems possible that the adaptive response of the human eye, coupled with a pseudo-stereo effect produced by satellite motion and short-term memory, might produce unexpectedly good observations of mesoscale meteorological or oceanographic features that are too large to be viewed from aircraft. Such observations, supplemented by high-resolution infrared and microwave sensors that man directs, might yield data of very good quality and utility. The use of man to select, evaluate, and reduce data to be transmitted to earth would require the development of specialized training as well as procedures to coordinate manned spacecraft with other observations.

SUMMARIES AND CONCLUSIONS

The main motivation behind public support for an earth-resources survey program is the prospect of an operational system for resources manage-

ment. Hence, although the earth-oriented observations are of significant scientific interest, a study based on scientific priorities alone becomes a somewhat artificial exercise. This strong difference of the earth-oriented observations from other parts of the NASA space sciences program makes it inappropriate to consider earth-oriented observations as an allocation of space science capability. The ideal context in which to determine allocations in support of earth-oriented satellite programs would be the overall federal R&D effort in resources and environment. The need to coordinate any space effort with airborne, surface, and data analysis and dissemination efforts also makes such a context preferable.

In the real world, however, NASA must take such arguments into account and use them to justify a budget, rather than wait for the allocation to be decided by some higher office. NASA will probably have to play the same leadership role in the development of the earth-resources use of satellites that it did in the development of the meteorological applications. In earth-resources surveys, however, much broader judgments must be brought to bear to attain proper balances between airborne and spaceborne techniques, between data collection and analysis, and between fundamental research and prototype development. All these judgments entail political and public relations problems beyond our competence to advise.

The principal recommendations of this Working Group are embodied in Table 7.

The meteorological program is scientifically sound and interesting and has high assurance of producing practical benefits. Hence it is the program that varies the least from one funding level to another.

The earth-physics program also has high scientific potential for oceanography and tectonics, but its practical benefits are further in the future. Thus its major component, the altimetric satellite, is included down to level 3 but not level 4. The varying ratio of SR&T to flight projects at levels 1 and 2 in Table 7 reflects the reduction of the very accurate tracking from ground stations to distant satellites and the moon.

The earth-resources program is the most speculative scientifically, even though it has the potentiality of the greatest practical benefit. Hence its funding level varies the most from the highest to the lowest level. Furthermore, the ratio of aircraft to flight project expenditure varies appreciably. This Working Group agrees that the greatest scientific and practical potential of space techniques lies in their use on problems of the biosphere. The biosphere is characterized by broad geographic extent and temporal variations and is best observed by sensors of high spectral (rather than spatial) resolution, which is facilitated by uniform solar illumination. All these factors make spacecraft remote sensing more effec-

tive relative to aircraft. For converse reasons, aircraft techniques are relatively more effective for remote sensing of geologic or cultural features. This Working Group unanimously concurred that space techniques are appropriate to survey the oceans, but for land survey opinions on whether they would be more effective than aircraft varied greatly; estimates as to the portion of the land for which aircraft would make operational surveys more efficiently than spacecraft ranged from 5 to 100 percent. It is unanimously agreed that the ERTS satellite should be launched in 1972, and that sizable aircraft programs should be flown at *all* funding levels in order to obtain the information needed to resolve these issues, as well as to aid directly in analyzing urban and biosphere problems.

Because of the greatly differing requirements for land and ocean spectrometry, an ongoing program in both areas is difficult to maintain below level 1. Although the oceans are inherently more appropriate for space techniques, we concur in giving greater emphasis to land-oriented observations because of the greater development of the applicable remote-sensing techniques, the greater potential of increased productivity of the land, and the greater urgency of land-associated problems.

This Working Group shares the general desire that the economic justification of an earth-resources survey program be analyzed. Studies in this direction should be carried out using data provided by the ERTS projects. However, we share the opinion of the 1969 study, *Useful Applications of Earth-Oriented Satellites*, that the use of satellites for earth-resources survey is at the stage of basic and exploratory research. The technology is advancing so rapidly that caution must accompany any attempts at economic appraisal, and conventional cost-benefit analyses are not feasible. A risk must be taken.

The earth-oriented use of space techniques has a peculiar urgency because of the hope it offers that an element of our technological capability can be turned to help man's resource and environmental problems. The potential benefits are sufficiently great that earth observations should be broadened even though we lack the clear definition of the scientific objectives normally desirable for such projects. Perhaps the greatest benefit will be to induce a less homocentric perspective of the environment, encouraging the view of man as a part of nature rather than the measure of all things. This view will be most sound if it is based on good science. Observations of the earth from space applies to fields where "good science" means more complete description, as in phenology, as well as to fields where the problems can be mathematicized more readily, as in meteorology.

9 Life Sciences

Biology is concerned with life in all its manifestations from molecular mechanisms to the dynamics of large populations, ecosystems, and the entire biosphere. The ultimate aim of the life sciences is to define the universal conditions of life, its origin, and its maintenance. Biological principles are basic to and relevant for application in medicine, population control, and the management of natural resources. In contrast with a field like astronomy which is a science of space and requires space vehicles to bring it closer to its principal objectives, the coupling of biology with space science is less obvious and more selective: as yet life is known only on this planet, and most biologists see as their primary task the investigation of terrestrial life in its natural setting. Yet, with man's venture into space, three new ways of looking at life have been opened to us. We can look outward to ascertain whether there is life on extraterrestrial bodies; we can look back from space to our own planet to observe, from this new vantage point, processes related to life on land and over the oceans; and we can study terrestrial organisms as they enter the space environment and ask how these organisms, including man, survive and perform under the unprecedented conditions of space: an all-pervasive factor—gravity—has been removed, and its role on earth can be fully assessed for the first time. Thus certain areas of the life sciences can contribute to, and benefit from, the space program in major ways.

We have divided these areas into five categories and listed under each a minimum of subtopics:

Members of this Working Group were D. Farner and H. Teuber, *Chairmen*; A. Brown, J. Ebert, B. Glass, J. Hastings, N. Horowitz, M. Menaker, N. Pace, A. Rich, C. Sagan, K. Thimann, W. Vishniac, and S. Warren.

1. *Exobiology* the attempt (a) to understand the origin of life on this earth; (b) to search for extraterrestrial life which would disprove the uniqueness of life as we know it; and, subsequently, (c) to learn what is common to *all* life and what is peculiar to terrestrial life.

2. *Earth ecology* the surveillance of the biosphere by satellite (or aircraft) monitoring of biological processes and related environmental factors. The surveillance possible from earth orbit can also help to resolve basic questions of animal orientation and navigation.

3. *Gravitational biology* the definition of the role of gravity in terrestrial life by exposing organisms to conditions in which the g factor can be virtually eliminated. Areas for study include gravity sensors and morphogenesis in animals and plants and, in animals (including man), metabolic, cardiovascular, and fluid-balance physiology.

4. *Behavioral biology* closely related to 3, the study of the behavior of higher organisms, ranging from alterations in activity cycles and sleep-wakefulness rhythms to basic questions of sensorimotor coordination, when terrestrial cues and influences are minimal or absent.

5. *Radiobiology* the determination of the reaction of animal tissues and, particularly, of nonregenerating tissues such as the mammalian central nervous system to the high-energy particulate radiation encountered on flights outside the shielding afforded by the terrestrial magnetosphere.

PRIORITIES IN SPACE LIFE SCIENCES

The order in which these five categories are ranked is our best estimate of the areas in which important observations or decisive experiments are at the moment most likely to emerge. Areas within categories are not ranked with respect to priority. Much like previous review groups in space life sciences, the present Working Group is unanimous in assigning to exobiology the highest overall priority, because all present biology is based on only one instance, the particular evolutionary sequence that has occurred here on earth. Defection of life elsewhere could transform the entire field of biology. Ground-based work, including studies of pre-biological chemical evolution and of terrestrial organisms that survive under extreme conditions, is well developed, and a long series of experiments has explored the formation of biochemically important compounds under conditions that we believe to be associated with the origin of life on the primitive earth. At the same time, the technology of life-detection systems, although inevitably based on life as we know it, has been brought to a point where useful questions about extraterrestrial life can and should be asked.

Should life be detected by missions such as the Viking flight to Mars, the discovery would be of incalculable general significance. Reliable negative results would also be of great moment, because they could show us at what stage chemical evolution stopped short of the origin of life on another planet; alternatively, fossil evidence might indicate an evolutionary biological sequence that has run its course.

Earth ecology, the second-highest item on our list, cannot be rated as high as exobiology in potential importance to basic biological science, but this is nearly outweighed by the far-reaching practical, social implications. We cannot assess the ultimate potential of surveying the terrestrial biosphere from orbiting satellites pending further advances in remote-sensing and data-processing techniques. Nevertheless, we anticipate that it will greatly facilitate the husbanding of threatened natural resources, the formulation and tests of countermeasures to large-scale pollution, and the assessment of the possibilities of a threatened runaway greenhouse effect and other gross man-made effects that could produce an uninhabitable planet. Progress in developing these new technologies happily parallels the upsurge of public interest in ecology. It also promises significant advances in ecological research.

Weightlessness is an unprecedented tool for fundamental biological research. Responses to gravity at $g > 1$ are being studied in animals and plants by means of centrifuges and form an important complement to prospective studies in space at $g \simeq 0$. From such and similar ground-based work it is clear that all terrestrial plants, and most animals, are gravity-sensitive. The mechanisms by which plants sense gravity are not yet known; in animals the sensors (insofar as they have been isolated) are much better defined, but their physiological role in maintaining posture and in sensorimotor coordination still raises questions of very general significance for biology and especially for neurophysiology. The effects of gravity on morphogenesis (development) of animals and plants and the subtle mechanisms that account for the effects are a field as yet virtually unexplored because the potentially most revealing experiments in this field can be done only when $g < 1$.

Much of our information on the effects of prolonged exposure to low g comes from manned flights. In the astronauts, cardiovascular changes in response to inertial forces, progressive decrease in bone mineralization and muscle mass, and rather drastic effects on fluid balance have been documented. None of these effects has been studied thoroughly or fully explained in terms of the underlying physiological mechanisms; they must be, if we are to be confident of man's ability to undertake long space missions. On the other hand, there appears to be a surprising resilience in human sensorimotor coordination under conditions of low and varying

g. In this case, the virtual absence of an expected effect forces one to revise traditional theories of vestibular function and basic coordination and indicates a series of ground-based and in-flight studies in experimental animals.

In this connection, we have asked ourselves whether biological experiments, on plants or animals, should be flown only when an astronaut can be in attendance. Some of us believe that there will always be crucial experiments that require the very-low-*g* levels obtainable only on unmanned, automated satellites where the *g* variations introduced by the astronaut himself are absent.

Next on our list is behavioral biology. The space environment provides the means to study the mechanisms of biological rhythms, which are basic to almost all living things, by divorcing them from terrestrial influences. Because evidence is accumulating that these rhythms are endogenous to the organism rather than entirely dependent on the environment, we do not assign as high a priority to in-flight experiments in this field as we would have a year ago; nevertheless, a thorough understanding of these rhythms is important for jet travel, work-rest cycles, and manned spaceflight (to give three examples). There is considerable practical impetus to ground-based and definitive in-flight experiments. The motivation for research into individual and small-group performance is primarily supportive of manned spaceflight—how to assure efficiency and compatibility during long periods of isolation and confinement in flight—but we think it not unlikely that it will also add to our basic knowledge in this field.

Radiobiology has considerable significance to the safety of astronauts in particular with respect to high-energy heavy (HZE) particles which may prove to be a serious hazard on long space missions. If only for these reasons, measurements and evaluation of space radiations must continue. We have, nevertheless, put radiobiology last on our list because we think it unlikely that it will markedly advance fundamental biological knowledge: we do not anticipate that the biological effects of space radiations, with the possible exception of HZE particles, will differ in kind from those encountered on earth. Moreover, past work in space life sciences, and especially the results of Biosatellite 2, dissipates the concern that weightlessness might aggravate the effects of radiation. In the absence of such synergism, the problem of HZE particles relative to astronaut safety would be accessible to ground-based attack if an accelerator capable of producing such particles were to be constructed.

It is evident that work in space biology can and must be approached along two lines: Space provides a *tool* for basic biological research and space represents a *task*—that of qualifying man for prolonged flights. These two aspects, the biological and the medical, are complementary,

in our view; neither of these two approaches can substitute for the other. It is this interdependence of the biological and medical concerns that leads us to recommend a reorganization of NASA's efforts in the life sciences. We have found it exceedingly difficult to come to grips with the wide range of activities, now dispersed throughout three major sections of NASA, *viz.*, the Office of Space Science and Applications, the Office of Advanced Research and Technology, and the Office of Manned Space Flight. For administrative and scientific reasons this split (which occurred in 1961) should be undone, in line with the urgent recommendations of previous review panels.

Such a recombination is doubly needed because it is already apparent that many aspects of man's physiological and behavioral responses to prolonged spaceflight will require complementary animal experiments. It is only in the experimental animal that certain invasive and histological procedures can be accomplished; and it is equally obvious that man must not be exposed to hazards that can be more safely assessed in lower forms. At the same time, major biological responses to the space environment can and must be studied in their own right, both for scientific reasons and for their ultimate potential value to advances in space medicine.

Finally, while this Working Group did not review specific manned spaceflight programs for the purpose of assessing the relative priorities of elements within them, we feel strongly that if prolonged manned spaceflights are to be undertaken, an extensive program of physiological, behavioral, and biomedical experiments and improvement in life-support systems are most important because these matters bear directly on the feasibility and success of such flights.

EXOBIOLGY

Exobiology, the topic to which we assign highest priority, is now at a stage where one should begin to implement the search for extraterrestrial life on Mars by a soft lander instrumented for life detection. This Working Group was not constituted to review and judge the individual Viking experiments in detail, but we have reached the unanimous opinion that these life-detection experiments are not likely to produce false positives, *i.e.*, to signal life where it does not in fact exist. On the other hand, we are constrained to point out that false negatives, *i.e.*, failure to detect life on a planet where some life does exist, are not improbable. The reasons are fairly obvious: as pointed out above, the experiments are necessarily designed to detect life as we know it; furthermore, sampling of

potential habitats for life on any single mission is necessarily restricted. Nevertheless, the problem of life on other planets remains a question of such overriding importance for our understanding of life on this earth and of life in general, that the "long-shot" nature of the Viking mission seems to us acceptable.

Biological exploration of Mars requires sterilization of the lander capsule. The cost of sterilization control and inspection, as well as quarantine-related investigations and development, is a fixed item of \$5 million per annum.

In addition to research and development directly related to Viking and funded by the Planetary Exploration program (see Chapter 3), this Working Group holds that the following exobiological topics should continue to receive support, in a balanced fashion, with Space Biology (SB, OSSA) Supporting Research and Technology funds:

1. Chemical evolution. The synthesis of organic matter from gas mixtures (putative primeval atmospheres) and nonbiological formation of biologically relevant compounds, including biopolymers, is the chemical foundation for the origin of life. It also has strong bearing on the origin in space of organic compounds the existence of which has been demonstrated by interstellar molecular absorption spectra.

2. Chemical paleontology. Closely related is the need for continuing organic analysis of ancient rocks including those of nonterrestrial origin. The methods used to detect amino acids in fossiliferous strata and in micropaleontology of pre-Cambrian rocks are now being applied to the analysis of lunar samples and meteoritic material. Ground-based facilities for receipt of planetary samples would have to be maintained.

3. Planetary ecology. An increasing number of ground-based studies have focused on the relationships between properties of living organisms and physical and chemical properties of their environment. This work should be extended beyond the 1975 Viking mission. The studies add to our understanding of the ability of terrestrial organisms to adapt to extreme environments, approaching those that may exist on other planets. In applied form, these studies are important because they bear on the problems of planetary quarantine.

4. Advanced exobiological instrumentation. While the preceding three classes of investigations are advancing without being necessarily related to individual flight programs, the need for developing new automated biological instruments for planetary studies is strictly mission-dependent. Hence the pace and size of such a development program should be adjusted to flight opportunities. It is this budget item that should absorb the bulk of fluctuations under various financial constraints.

The above recommendations regarding ground-based work in exobiology may be modified in the direction of increased activity and funding if the Viking mission should lead to a positive identification of biological material on Mars.

EARTH ECOLOGY

Surveillance of the terrestrial biosphere from aircraft and orbiting satellites holds promise of such far-reaching social benefits that this Working Group has accorded it second-highest priority. We make this judgment in full awareness of the fact that the ultimate potential of this area cannot be assessed at present. The immediate tasks, and the major claims for funding, lie in the further development of multispectral and other sensors and of methods for data processing. We recommend that support for these developments continue at least at the current level, and we expect that, with advances in technology, increased support will be merited in the future.

We believe that earth-orbiting satellites and specially equipped research aircraft can further ecological studies primarily in two ways: as platforms for specialized sensors (e.g., multispectral spectrometers that view the earth with a perspective otherwise not attainable) and as communication systems between the biosystems under study on earth (e.g., wildlife species) and the investigator who seeks information on these animals in their natural habitat over long periods and distances. In many cases such data do not seem to be obtainable by any other means.

For both these purposes, although existing technology can make useful contributions and should be exploited, major improvement in instrumentation and data-processing techniques is needed. The detecting and resolving power of multispectral sensors must be increased and the capacity of computer systems for data processing considerably expanded. For animal tracking, miniaturization of transponder systems to the point where they can be carried by smaller animals such as migratory birds seems to us essential before the full impact of these new methods can be felt.

The general research program, together with the associated technological developments, should be guided by such questions as:

1. What are the biological signatures that can be recognized remotely in the visible, ultraviolet, and infrared wavelengths with which we can predict biological response to environmental change? Can we recognize slight changes in phosphate and other biologically important anions and

cations in rivers, lakes, and bays from aircraft or satellites? Can changes in organic matter (hydrocarbons), inorganic salts, and pH be detected? What are the signatures that indicate insect invasions, plant diseases, specialized ecological systems?

2. What factors in biosphere-atmosphere interaction can help to predict long-term atmospheric and surface changes on a planet-wide scale? Can we relate long-term changes in temperature to biological modifications of the atmosphere? Will one of the following alternatives confront us in the decades ahead: (a) increasing concentration of carbon dioxide in the atmosphere as a result of biological activity, with progressive increase in surface temperatures (the greenhouse effect), or (b) progressive decrease in surface temperature (as has been happening for the past 24 years), presumably because of an accumulation of particulate matter in the atmosphere from fossil-fuel combustion and subsequent decrease of solar energy reaching the surface?

3. With respect to animal tracking and monitoring, how and under what circumstances do animals migrate? What physiological changes occur during migration (e.g., water loss, vascular changes, energy balance)? How do animals orient and navigate?

Assuming that equipment can in fact be sufficiently miniaturized to be carried by the animals under study, the tools would be in hand for the first time to attack some of the greatest riddles of animal behavior, of practical importance to resource management and conservation. (For a detailed treatment of this topic and a discussion of the relative merits of satellites and aircraft for such monitoring, see *Space Biology*, Chapter 6.)

GRAVITATIONAL BIOLOGY

Studies in gravitational biology are undertaken for two reasons: to advance our basic knowledge of the role of gravity in terrestrial organisms, including man, and to assure the ability of man to tolerate long periods in spaceflight and to return to 1 g without ill effect.

Gravity is an ever-present and often dominant environmental influence on living things. How organisms grow and develop on earth is profoundly influenced by gravity—(a) in the larger animals and plants because it presents a persistent mechanical stress to be overcome and (b) in nearly all organisms because they use it for orientation if not in other ways. The form of many organisms, especially of the higher plants, is fundamentally affected by the directional clues they obtain by sensing the gravitational vector. An immediate goal of basic gravitational biology is thus to learn the mechanisms by which organisms sense the gravitational force field.

Especially in higher plants, these mechanisms are poorly understood. The sensors are not physically identified, and even the principle of their operation is not certain despite extensive work at $1 \geq g$. The field needs a full quantitative description of the overall stimulus-response process. Salient questions aim at determination of response kinetics. For a complete description the condition $1 > g \geq 0$ must be explored, not merely $g = 0$. A second goal is to determine the effects of gravity on the growth and development of plants and animals, combining data from experiments at $g > 1$ with experiments at low- g levels.

Studies of the effects of gravity on higher animals, including man, are particularly relevant to the practical considerations of manned spaceflight but are also expected to contribute to basic knowledge. Protracted exposure to low g has been shown, in animals and man, to produce three major biological effects: changes in fluid and electrolyte balance, changes in cardiovascular function, and changes in skeletal structure and muscle mass. There are several other changes, primarily observed in astronauts, for which the evidence, and the relation to weightlessness as a principal causal factor, are less clear. These include hematological changes and vestibular effects. The alterations in fluid and electrolyte balance are among the most urgent topics for investigation. The mechanism is not understood, but progressive fluid and electrolyte loss, with increasing lengths of spaceflights, has now been observed in dogs, subhuman primates, and man. Attempts at extrapolation (admittedly hazardous) from the data now available, with corrections for differences in the ratios between body surface and volume, have led some investigators to predict that the progressive shift of fluids from the tissues into the vascular bed could reach dangerous proportions in manned flight by 21 days unless countermeasures are instituted. Animal experiments on the mechanisms and countermeasures, extensively on the ground and critically during prolonged flights, seem to us most urgently needed.

The same can be said for the changes in cardiovascular reactivity observed in animal and man under spaceflight conditions. The Biosatellite 3 primate clearly demonstrated a rapid increase in venous pressure under weightlessness. In astronauts, the slowing of heartbeat during flight and orthostatic hypotension following return to $1 g$ (with rapid recovery) are well known. What is not known is the relation between the cardiovascular and the fluid balance changes; nor are we clear on the extent and limits of the cardiovascular changes, which may be a form of acclimatization to the space environment, attesting to the remarkable adaptive capacities of homeostatic mechanisms in higher species.

Equally firm is the evidence for changes in bone density and muscle mass. Total bed rest mimics this phenomenon, but the rate of mineral

loss from the bones is considerably more rapid in weightlessness. Again, the mechanism is not understood in either case, and the question is how far these changes will progress with time in space. The relative importance of immobility and of low *g per se* in producing the changes must be worked out; both ground-based and space experiments are necessary to reach definite conclusions. The flying of small animals, some under enforced exercise and others as controls, in a recoverable vehicle, is one of the main ways to obtain needed results.

Hematological changes and vestibular effects (the poorly understood and unpredictable instances of motion sickness in some American and Russian astronauts) have been attributed to weightlessness, but this is by no means certain. It is argued, for example, that changes in red-blood-cell mass could be due to the oxygen atmosphere, reduced exercise, or environmental factors other than weightlessness, singly or in combination. Systematic exploration of all these possibilities by ground-based work followed by critical animal experiments in space seems to us indicated. The same is true for the claimed changes in immune response. Here, further observations in manned flight should precede any decision on animal experiments. The situation with regard to low-*g* effects on vestibular functions, however, seems to us rather different. Here, because implanted probes may be necessary to understand the effects, prior animal experiments may be necessary.

The far-reaching adaptation of the sensorimotor system to weightlessness is remarkable and unexpected. Prevailing views on the role of the gravity sensors in animals and man in controlling posture and mediating sensorimotor coordination would have led to dire predictions concerning spaceflight. In our view, this absence of expected trouble requires just as much explanation as the occasional presence of trouble; ground-based work on the neurophysiology of sensorimotor coordination and the role of the vestibular sense and other proprioceptive modalities should be undertaken with this remarkable adaptability in mind. Flight experiments on animals under very low-*g* conditions, with test periods exposing the animal to brief linear or angular acceleration, would also have considerable value, regardless of whether the animal's total-body motor reactions are being monitored (as in currently proposed experiments on fish) or whether the results are obtained via microelectrodes implanted in the inner ear or vestibular nerve (as in the Scout frog-otolith experiment). It should be noted that experiments of this type can be flown on non-recoverable vehicles, with restartable boosters (to provide periods of linear acceleration) or small on-board centrifuges.

Running through all discussions of gravitational biology is the question of the degree to which 0 *g* can be simulated. This question is of first

importance, if only because flight experiments can be 1000 times more expensive than ground-based experiments. We begin by saying that the question is not resolved, and that we differ among ourselves in our responses to it. On certain points we agree.

Clinostats, liquid immersion, bed rest, and ballistic flights have been used to simulate certain aspects of weightlessness. Each is limited in its usefulness and applicability. Thus the clinostat is useful only in experiments with systems responding to long presentation times (e.g., plants); immersion induces physiological responses quite unrelated to 0 *g* and hence cannot serve as an adequate model of weightlessness; bedrest neutralizes the important vertical component of gravity but obviously does not do away with gravity; and the free-fall condition in ballistic flights is limited to tens of seconds. The central question is to what degree does a particular method of simulation produce the same biological results as are obtained in spaceflight. The responses of most biological systems to 0 *g* are not known, so the value of methods for simulating 0 *g* is still an open question and must remain so until spaceflight experience offers sufficient basis for generalization. Prior to spaceflight of biological materials all seemingly appropriate simulation studies should be carried out to suggest to the experimenter what sort of effects might be encountered, to improve the scientific value of flight observations, and to test further the adequacy of particular simulation techniques.

On the other hand—in spaceflight—it has not always been possible to differentiate conclusively between the effects of weightlessness and those of other environmental factors such as vibration and acceleration at launch and re-entry. Here, again, ground-based experiments are called for. Increased work on the biological effects of physical–environmental factors other than weightlessness seems to us urgently indicated, both in preparation for future flights and to make maximal use of the information obtained thus far.

Finally, it is impossible to simulate prolonged 0 *g*; there are classes of experiments that must be conducted in spaceflight. We have touched on the most obvious ones above.

BEHAVIORAL BIOLOGY

Behavioral biology focuses on the effects of spaceflight upon the more complex aspects of coordinated activity in higher organisms, such as perception and skilled movement, general orientation and arousal, motivation and capacity for learning, and social interaction. All these aspects of behavior are put under an unusual strain by the conditions typical of

spaceflight: weightlessness, vibration and unusual motion, sensory isolation and deprivation, confinement and motor restriction, radiation, loss or drastic modification in time reference, and, in the case of manned flight, the stress of command decisions. Most of these factors, except prolonged weightlessness, can be at least partly replicated on the ground, but to study them all in combination will require actual spaceflights.

1. **Biological rhythms.** The role of geophysical factors, such as solar and lunar cycles, as time-setters for biological rhythms is well known. These rhythms express themselves in many species and on many levels, from approximately diurnal cycles (circadian rhythms) in various metabolic activities and body temperature to the periodicities of sleep and wakefulness, motor activity and feeding, and the variations in response to drugs as a function of time within the circadian period. On a longer time scale, many animals, especially marine invertebrates, show a strict relationship between the lunar cycle and their reproductive cycles. Yearly rhythms express themselves in the reproductive activities of vertebrates from fish to mammal and in changes in growth and differentiation rates among photosynthetic organisms even when they are kept under constant conditions.

Most investigators now believe that these phenomena can only be understood in terms of some underlying endogenous biological clock, particularly because ground-based experiments, in which most time-setting influences have been removed, have shown that the basic rhythms continue in a free-running fashion, often getting out of phase with those geophysical periodicities from which the organism has been shielded. Thus, rapid geographic displacement of organisms across time zones (as in jet travel) often results in malfunction, because the various rhythms (from metabolic patterns to problem-solving capacity) tend to entrain to the new environment at different rates. Much of this problem can be investigated by carefully designed experiments on the ground and in air flight.

Some investigators consider that the widely accepted idea that biologic clocks are of endogenous origin is unproven until the organisms showing such cyclic behavior have been effectively removed from all conceivable geophysical influences. These investigators deem it mandatory that organisms be flown in high elliptical orbits or in deep-space probes. The majority of our Working Group agrees that biological rhythms should be studied in space, as in the approved Skylab A experiments, even though that vehicle will be in circular earth orbit.

Studies of biological rhythms in space are also important to the success and optimization of manned spaceflight. In the long run one would want to investigate these periodic changes in function in the astronaut, but

there are, at least at present, severe limits on the experimental regimens that can be imposed on man during spaceflight. Here animal flight experiments would be desirable.

2. Sensorimotor coordination. Quantitative studies of coordinated motor activity, acquisition of skills, learning, and problem solving, as they apply to the space environment and its demands, are important for manned flights. The physiological aspects of problems of coordination under weightlessness have been discussed earlier, but it remains to be seen how far the unloading of the vestibular organ during prolonged exposure to low g would influence the capacity of higher organisms to maintain alertness (let alone sleep-wakefulness cycles) and appropriately motivated states.

The potentially disruptive consequences of relatively monotonous sensory inputs during space travel and the less definable aspects of sensory isolation may have been overestimated. Ground-based work can duplicate many of these conditions (except for the deprivation of otolith input and other proprioceptive sources). The effects of motor deprivation, i.e., the lowered opportunity for normal locomotion, may have received less attention than they deserve. More carefully considered behavioral experiments, preferably on man, seem to us indicated, including quantitative studies of acquisition and maintenance of learned skills (now planned for Skylab, shuttles, and space stations). Perceptual-motor tests involving judgment of "verticality" under low- g conditions are likewise important, because they may contribute to our understanding of the interactions between perception, orientation, and the control of posture.

3. Social behavior. The importance of personal and interpersonal reactions to the environmental and psychological conditions of prolonged spaceflight is beginning to be appreciated as a cause for possibly serious disruption of manned missions. The principles being developed by the experimental and quantitative study of social interactions will help to determine the size of groups most effective for performing certain tasks and to decide what elements must be provided for leadership and for ways of averting hostile and aggressive interactions. Nevertheless, the circumstances of long space missions have no close analog on earth, and space may provide a unique laboratory for basic insights into social-psychological problems which have always plagued mankind.

RADIOBIOLOGY

The flux of high-energy particles of high atomic number (HZE radiation) is, like prolonged weightlessness, a unique feature of the space environ-

ment. So far these particles cannot be produced by ground-based accelerators; they are encountered, albeit in small numbers, on balloon flights. Monkeys exposed in balloons at altitudes around 12,000 ft have been examined *post mortem* for brain damage, but neuropathologists are still divided in their interpretation of the evident brain cell losses and inflammatory reactions. The difficulty lies in the possibility that the observed changes either preceded high-altitude exposure or were secondary results of the irradiation of body parts other than the brain.

Information on the extent of such possible hazards is urgently needed if prolonged manned missions are to be undertaken. From currently available estimates, the uncertainty about the possible loss of brain cells in astronauts on a two-year round trip to Mars range from 1% to 100% of all neurons in the cerebral hemispheres. On lunar missions, astronauts have reported frequent subjective light flashes and luminous streaks which might represent HZE hits in either the retina or visual cortex. No after-effects in the form of visual-field defects or other abnormalities have been detected.

The space biology program has already demonstrated that a synergism between weightlessness and sparsely ionizing radiation is highly improbable. In fact, some members of the Working Group regard this demonstration as one of the major contributions of the U.S. space biology program.

If one accepts the apparent absence of synergism between weightlessness and radiation, then the obviously preferred option for assessing the effects of HZE particles is to build a ground-based accelerator capable of producing them and to expose test animals and tissues to known fluxes of the radiation. There is hope that such an accelerator will be made available by the Atomic Energy Commission as a national facility. In that case, ground-based work should be funded for further development of physiological and biological dosimetry and for intensive investigation of neurological and behavioral changes incurred in animals by HZE-particle exposure. However, should such an accelerator not become available, a series of animal flights, preferably beyond the terrestrial magnetosphere (or, as possible alternatives, deployment of automated biopackages on the moon or long-term experiments on Skylab) should be considered most seriously.

We have concentrated here on the importance of research on HZE particles; we do not wish to imply thereby that these particles represent the only space radiation hazard—simply that they are the least understood. We endorse the recommendations of the Santa Cruz study (*Space Biology*, pp. 6, 45) for continued and extended in-flight measurements and monitoring of radiation dose, type, and quality and depth-dose relationships.

PROGRAM OPTIONS

The foregoing discussions have treated the areas of life sciences relevant to the space program without reference to the ways in which they are administratively divided within NASA for funding. Our Working Group was requested by the study's Executive Committee to concentrate on those programs supported by the Space Biology Program of the Office of Space Science and Applications and to confine its specific budgetary recommendations to those programs, i.e., SR&T for exobiology and ecology, and SR&T and flight programs for gravitational and behavioral biology and radiobiology. The fact that the administrative and operational fragmentation of the NASA life-sciences programs constrains us to an arbitrary and bizarre separation of complementary efforts is one more evidence of the need for centralization of the NASA life sciences.

This Working Group favors a balanced program in space biology, based on a continuing and increasing SR&T effort in all five areas; the majority also favors a flight program that would accommodate at least a minimum of those projects that require flight. We recommend, however, that the decisions about such programs be made only *after* the total life-sciences effort within NASA has been reorganized. We must caution against any realignment that would cause the orientation of space biology toward basic science to be lost. The advice of the Glass Committee (*Life Sciences in Space*) on NASA organization should be carefully considered before any irreversible action is set in motion. Programs recommended by the majority are shown at two budgetary levels, \$35 million and \$55 million per year average for 10 years, in Figures 1 and 2. These present in graphic form our ten-year projections for SR&T, for definition of experiments for manned spaceflights,* and for a series of Improved Biosatellites for experiments primarily in gravitational biology. Planetary quarantine is a fixed item at \$5 million per year.

The \$55 million level permits a more extensive effort in ground-based support and allows far better advantage to be taken of the opportunities for biological research afforded by Skylab and follow-on manned missions. A series of six Improved Biosatellites is possible at the \$55 million level as compared with four at the \$35 million level. We consider this vehicle preferable to the standard Biosatellite and to nonrecoverable Biosatellites for all unmanned experiments except possibly a deep-space study of biological rhythms; the option for such a Bioexplorer mission should remain open pending results of the Skylab rhythm experiments.

At budgetary levels below \$35 million, no unmanned flight program is possible. SR&T work will suffer in the absence of a flight program, and the

* Following approval for flight, funding of such experiments is assumed by Office of Manned Space Flight.

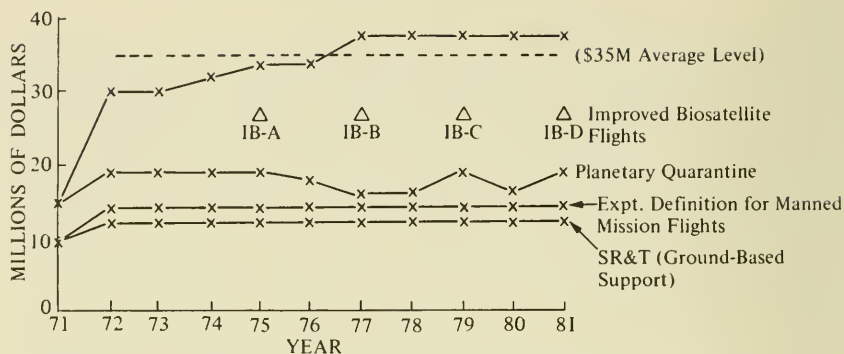


FIGURE 1. ossa space biology program—\$35 million annual average level.

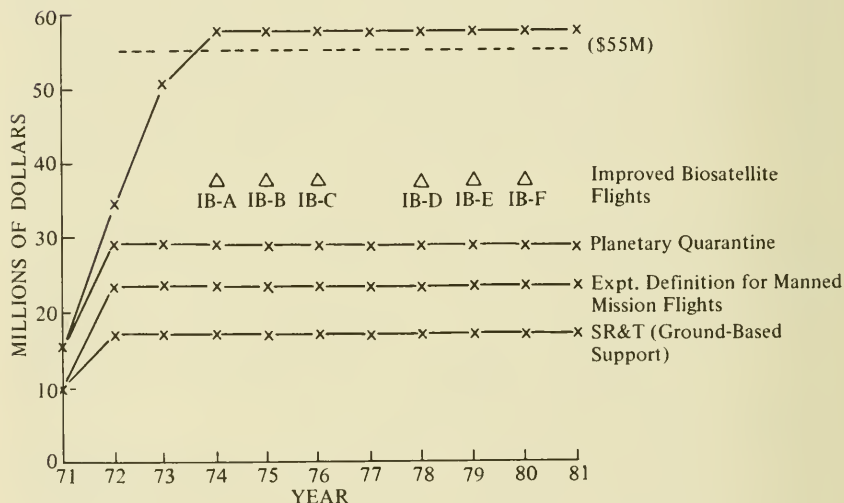


FIGURE 2. ossa space biology program—\$55 million annual average level.

space biology program will remain viable only if experiment definition for manned flight opportunities is vigorously pursued. Withdrawal of all potential flight opportunities in the coming decade could be tantamount to abandoning the field altogether. We are agreed that NASA's responsibility for biological science in space should continue, and we propose that it should receive increasing attention and support.

APPENDIX

A Earlier Reports

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